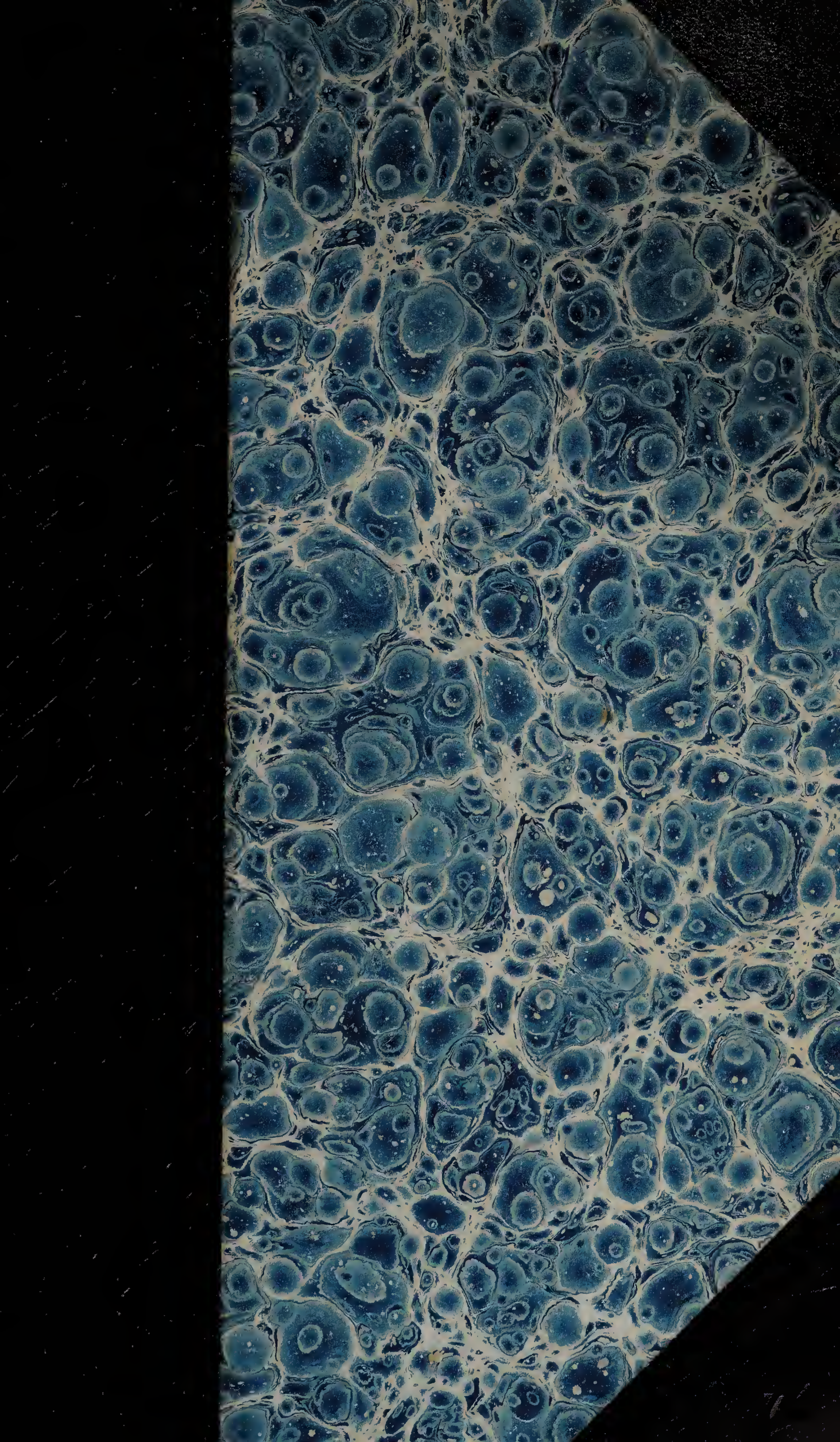

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MEMOIRS
OF THE
Royal Society;

Being a New ABRIDGMENT of the

Philosophical Transactions.

Giving an ACCOUNT of the Undertakings, Studies, and Labours of the LEARNED and INGENIOUS in many considerable Parts of the WORLD; from the first Institution of that ILLUSTRIOUS SOCIETY in the Year 1665, to the Year of our LORD 1735 inclusive.

The whole carefully abridg'd from the Originals, and the Order of Time regularly observ'd, with a Translation of the LATIN TRACTS, and the Theoretical Parts apply'd to Practical Uses; also an Explanation of the Terms of ART as they occur in the Course of the Work. Being a Work of general Use to the Publick, and worthy the Perusal of all MATHEMATICIANS, ARTIFICERS, TRADESMEN, &c. for their Improvement in various Branches of Business.

by Mr. BADDAM.

VOL. X.

Illustrated with a great Variety of COPPER PLATES.

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M,DCC,XLI.

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MEMOIRS
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ROYAL SOCIETY;
Being a New ABRIDGMENT of the
PHILOSOPHICAL TRANSACTIONS.

Experiments on the Indian *Magnetic Sand*; *by M.* Muſchenbroek. Philoſophical Tranſactions N° 432. p. 297.

THE *Indian* ſand, which is brought to *Holland*, is ſaid to be chiefly gather'd upon the ſea-ſhore in *Perſia*; after which it is boil'd in water, to free it of its ſaltneſs; and after this it becomes a black powder, conſiſting of grains of different bigneſs; ſome of which have a very rough ſurface, and others one part of their ſurface ſomething rough, and the other very ſhining: Their figure is very irregular, like grains of common ſand; only this *Indian* ſand is ſmaller. Theſe little lumps have neither taſte nor ſmell, and are friable; ſo as to be eaſily reduced to a very ſubtile powder. It has ſome parts, which are ſtrongly attracted by the loadſtone; and others ſo very unactive, as ſcarcely to ſeem to be magnetical: The blackeſt are the ſtrongeſt, but the unactive ones are more ſhining, and inclining to the colour of lead; theſe are in the greateſt quantity; and from them the others are got out by a loadſtone. The ingenious *Montenus* has ſeveral ways examin'd ſuch a kind of ſand, which is brought from *Virginia*,

and described it in *Phil. Trans.* N° 197. M. *Muschenbroek* examin'd the *Indian* sand another way; of which he gave an account in his *Physical Dissertations*, p. 127. But a great deal still remain'd to be considered; and as there is a great deal more of this substance of the lazy or unactive than of the active or magnetic sort; it was proper to try, whether a magnetic virtue might not be excited or increas'd in all of it; and after a few trials he found the thing succeed. He suspected that there might perhaps be too great a quantity of sulphur adhering to the sand, to suffer it to be turned into any metalline *regulus* by a long continuance in the fire; he therefore, toasted it in an open crucible for two hours with half the quantity of pot-ash; he afterwards wash'd away the salt with water, and the sand remain'd much blacker than before, of which he found more than a quarter endued with a greater magnetic force. M. *Muschenbroek* does not scruple to attribute this virtue to the salt; because tho' the action of the fire alone does increase the force of the sand; yet it does not give it near so much attractive force.

And because common black soap is made of oil boil'd with a *lixivium* of pot-ash, he had a mind to try whether soap might not do more than salt alone in exciting the virtue in the sand: So he mix'd the sand with an equal quantity of soap, which he first expos'd to a gentle fire in an open crucible, to dry up the soap which swells very much; then the fire was heightened for $\frac{3}{4}$ of an hour, the oily substance entirely consumed, and the matter in the crucible strongly fir'd: Then afterwards boiling it in water, and washing it well, he obtain'd a black sand, which was all endu'd with a vigorous attracting force. Pleas'd with this success, he had a mind to try whether he could excite a greater force in it: Wherefore he again roasted it with black soap as before, and even a third time; but no addition was thereby made to its virtue. He found that keeping it too long in the fire is as prejudicial as keeping it too short a time; between half an hour and an hour seem'd to him the most proper space of time.

He afterwards added to the black soap half the quantity of salt of tartar, mixing therewith an equal quantity of sand which, when expos'd in a crucible to a reverberatory fire for $\frac{3}{4}$ of an hour, he wash'd in water; and then so considerable was the virtue of the sand, that if it did not exceed the former, it at least was equal to it.

And becauſe he had obſerv'd the oilineſs of the ſoap to conduce much to excite the virtue in the ſand; he mixt beef-tallow with an equal quantity of ſand, and having cloſed the crucible very well, he expos'd the whole maſs to a reverberatory fire for two hours; whereby the ſand became much blacker, and receiv'd a great deal of attractive virtue: But that ſand, which was burned two hours with an equal quantity of pitch, became more active, as alſo very black, ſubtile, and very little ſhining: But when it was expos'd a longer time in the ſame crucible, he obſerv'd it to be weaker; as alſo when it was in the crucible with the pitch, but $\frac{1}{8}$ of an hour, it ſcarce acquir'd any virtue: So that there muſt be a determined action of fire to raiſe the virtue in the ſand. Yet he could not excite a greater virtue in the ſand than by the following means; *viz.* mixing the ſand in the crucible with equal parts of roſin, pitch, frankincenſe, and rape-oil, and expoſing it to a reverberatory fire for an hour, having firſt clos'd up the crucible well. Between the black coals of the oily matter, there ſticks a very black ſand which leaps up ſwiftly to the loadſtone, as ſoon as it is brought near it. Then he conſidered whether the ſand did not acquire the greateſt force, as it came nearer to the nature of ſteel, by burning it with the above-mentioned bodies; and ſuſpecting this, in order to try it, he put it among ſuch bodies as turn iron into ſteel, according to the operations deſcrib'd by that great Experimenter M. *Reaumur*, in that excellent book, entituled, *The Art of turning Iron into Steel.* He, therefore, took three parts of ſand, two parts of chimney ſoot, and of ſea-ſalt, powder'd charcoal, and aſhes, one part each. Having accurately mixed all theſe bodies together, they were expos'd for ſix hours in a cloſe crucible to a ſtrong fire; and then the whole maſs was boil'd and waſh'd in water, then dried, and ſo receiv'd a great deal of attracting force: but it was not near ſo active as that prepar'd with ſoap, or in the manner laſt deſcribed.

M. *Muſchenbroek* conjectured at firſt, that this ſand is an imperfect magnet, or ſubtile powder of it, which, when it is grown up into a greater lump, forms the common loadſtones: But when he found by experience that common loadſtones expos'd to the fire, according to ſome of the methods above-mentioned, did rather loſe of their force than gain, he alter'd his opinion, and owns that he has not hitherto penetrated into the knowledge of this matter.

What-

Whatever it be, it is certain that there are ſeveral kinds of this ſand, brought from different countries of the earth: For, it is brought from *Perſia*; and ſome from *Virginia*: There is another ſort in *Italy*, common enough at *Leghorn*: And this laſt is naturally very attractive: There are two ſorts found in the *Eber*, a river of *Haſſia*; of which one reſembles the *Italian*, and the other conſiſts of large grains, almoſt as big as hemp-ſeed, but ſcarce having any virtue.

M. *Muſchenbroek* had, beſides, a very vigorous ſort, which he is told was got near old *Raguſa* in *Dalmatia*. The ſeveral kinds of this ſand are unknown, which time and the diligent obſervations of philoſophers muſt hereafter ſhew.

Some Obſervations made in London; *by Mr.* George Graham; *and at* Black-river *in* Jamaica; *by Mr.* Colin Campbell, *about the going of a* Clock; *in order to determine the difference between the lengths of* Iſochronal Pendulums *in thoſe Places*; *and communicated by Mr.* Bradley. Phil. Tranſ. N° 432. p. 302.

THO' it be upwards of 60 years, ſince M. *Richer* firſt diſcover'd, that pendulum's of the ſame length do not perform their vibrations in equal times in different latitudes; and tho' ſeveral experiments, ſince made in different parts of the earth, concur to prove, that pendulum's ſwinging ſeconds are in general ſhorter as we approach the equator: Yet what the real difference is between their lengths in different latitudes, does not ſeem to have been determin'd with ſufficient exactneſs by the obſervations that have hitherto been communicated to the public; as may be gather'd from the 20th. propoſition of the third book of Sir *Iſaac Newton's Principia*, where they are compar'd as well with each other, as with the theory of that illuſtrious author. It were, therefore, to be wiſh'd that more of this kind of experiments could be made with greater accuracy in proper places, by ſuch perſons as have ſufficient ſkill and opportunities to do it; that we might thereby be enabled to judge with more certainty concerning the true figure of the earth, and the nature of its conſtituent parts.

As an inducement to ſuch as may have it in their power to put the like again into practice, here follows an account of a very curious experiment of this ſort, made in *Jamaica* by Mr. *Campbell*: His clock, which was made by the ingenious Mr. *Graham*, was ſo carefully contriv'd, that its pendulum might at pleaſure be reduced to the ſame length, whenever there ſhould

ſhould be occaſion to remove the clock from one place, and ſet it up in another.

This clock, being chiefly deſigned for aſtronomical obſervations, had no ſtriking part, and its pendulum was adjuſted to ſuch a length, that in *London* it vibrated ſeconds of ſidereal, and not ſolar, time. When it was finiſh'd, Mr. *Graham* fix'd it up in a room, ſituated backward from the ſtreet, and on the northſide of his houſe to prevent its being diſturbed by coaches, or other carriages that paſs'd thro' the ſtreets, and that it might be as little affected by the ſun as poſſible. Having ſet it going, he compared it with the tranſits of the ſtar *lucida aquilæ* over the meridian, which paſs'd

	Days		h	′	″	
Auguſt 1731	20th	at	8	59	15	by the clock.
	22	at	8	59	18	
	23	at	8	59	20 ½	
	25	at	8	59	22	
	28	at	8	59	25 ½	
	29	at	8	59	26	
	30	at	8	59	27	

Hence it appears that the clock gained 12 ſeconds in 10 apparent revolutions of the ſtar.

In order to eſtimate how much the pendulum may be lengthened by greater degrees of heat, or how much ſlower the clock would go on that account, when remov'd into a warmer climate, a thermometer was fixed by the ſide of it; and between the hours of 10 and 11 o'clock in the morning, and at night, notice was taken at what height the ſpirits ſtood, and the mean height for each day was as follows.

	Days	Therm.
Auguſt 1731	21	32 ½ Diviſions.
	22	30 ¾
	23	28 ¾
	24	27 ¾
	25	28 ¼
	26	27 ¼
	27	27 ½
	28	27 ½
	29	27 ½
	30	27 ¾

Hence

Hence the mean height for all theſe days was about 28 ½ diviſions.

The clock-weight, that keeps the pendulum in motion, is 12 *lb.* 10 ½ *oz.* and is to be wound up once a month. The weight of the pendulum itſelf is 17 pounds; and (during the time that the clock was compar'd with the tranſits of the ſtar) it vibrated each way from the perpendicular 1° 45′. The magnitude of the vibrations was eſtimated by means of a braſs arch, which was fixed juſt under the lower end of the rod of the pendulum, and divided into degrees, *&c.*

Auguſt 31, Mr. *Graham* took off the weight belonging to the clock, and hung on another of *6 lb.* 3 *oz.* and with this weight the pendulum vibrated only 1° 15′ on each ſide; and the clock went 1 ſecond and ½ ſlower in 24 hours, than when its own weight of 12 *lb.* 10 ½ *oz.* was hung on.

This experiment ſhews that a ſmall difference in the arcs, deſcribed by the pendulum, or a ſmall alteration in the weight that keeps it in motion, will cauſe no great difference in the duration of the vibrations; and therefore a little alteration in the tenacity of the oil upon the pivots, or in the foulneſs of the clock, will not cauſe it to accelerate, or retard its motion ſenſibly: From whence we may conclude, that whatever difference there ſhall appear to be, between the going of the clock at *London* and in *Jamaica*, it muſt be entirely owing to the lengthning of the pendulum by heat, and the diminution of the force of gravity upon it.

Mr. *Graham* ſent very full directions to Mr. *Campbell*, deſcribing in what manner the clock was to be fixed up, and how the pendulum might be reduced exactly to the ſame ſtate as it was when in *England*; but no intimation was given concerning the going of the clock, that the experiment might be made with all poſſible care, and without any byaſs or prejudice in favour of any hypotheſis, or former obſervations.

In *July* 1732 we receiv'd an account of the ſucceſs of the experiment by the hands of Mr. *Joſeph Harris*, who was preſent at the making of it in *Jamaica*; and who brought over with him the original journal of the obſervations of the tranſits of 2 ſtars, (*viz. Sirius* and β *Canis majoris*) over the meridian, compared with the clock, after it was fixed up in *Jamaica*, as Mr. *Graham* had directed; together with the height of the ſpirits of the aforeſaid thermometer, on the ſeveral days of obſervation.

The

The chief of those observations are contain'd in the following table; the first column shews the day of the month; the second the name of the star, and the time by the clock of its observed transit over the meridian; the third contains the hour of the day, when the thermometer was observ'd, together with the height of the spirits at those hours; the morning hours being denoted by the letter A, and those of the afternoon by the letter P.

1732	Canis Majoris. Time of Transit.	Hour of Day.	Thermometer.
Jan.	h ′ ″	h	
23	β 11 59 50	10½ A	14¾
	α 12 22 14	9½ P	11
24	Cloudy.	11½ A	15¼
25	β 11 55 40	8½ A	17½
	α 12 18 4	9¼ P	11¼
26	β 11 53 35	8 A	20
	α 12 16 00	2 P	8½
		9 P	10
27	β 11 51 31	7 A	17½
	α 12 13 55	2 P	8½
		9¾ P	12½
28	β 11 49 26	7 A	20½
	α 12 11 51	2 P	11
		10 P	12
29	β 11 47 22	6¾ A	19
	α 12 9 46	3 P	9
		9 P	11¼
		7 A	20½
30	Cloudy.	4 P	7
		11 P	13
31	β 11 43 12	7 A	20
	α 12 5 37	9 P	8½
Feb.	β 11 41 8½	10 A	18¾
1	α 12 3 33	11 P	16

1732	Canis Majoris. Time of Transit.	Hour of Day.	Thermometer.
Feb.	β 11 39 0	9½ A	17½
2	12 12 3½	2 P	9
	α	5 P	6
		9 P	8½
3	β 11 36 53	8½ A	19
		1 P	9½
		9 P	9
4	β 11 34 46	6¼ A	18
	α 11 57 11	12	9½
		9 P	8
5	β 11 32 40	7½ A	19¼
	α 11 55 5	3½ P	6
		8½ P	8
6	β 11 30 35	7 A	18½
	α Cloudy.	4 P	7½
		8½ P	8
7	β 11 28 31	7 A	20½
	α 11 50 55	12	12
		8½ P	8½
8	β Cloudy.	6½ A	21½
	α 11 48 50	8½ P	8¼
9	β 11 24 20	9½ A	14
	α 11 46 44	8½ P	8
10	β 11 22 12½	7¼ A	16
	α 11 44 37	11½ A	10
		3¾ P	3¾
		8½ P	6

1732	Canis Majoris.	Time of Tranſit.	Hour of Day.	Thermometer.
Feb.	β	11 20 6	7½ A	16
11	α	11 42 30	12	9½
			8½ P	5¾
12	β	11 18 0	10 A	17½
	α	11 40 24	12	13
			8 P	5¾
13		Clouds.	9 A	17
			8 P	6
14	β	Cloudy.	7½ A	16
	α	11 36 15	12	11
			8 P	10
15		Clouds.	9 A	18
			12	13¾
			8½ P	7½

1732	Canis Majoris.	Time of Tranſit.	Hour of Day.	Thermometer.
Feb.	β	Cloudy.	8 A	14
16	α	11 32 4	8 P	7
17	β	11 7 34	12	12
	α	11 29 59	8 P	6¼
18	β	11 5 29	12	12¼
	α	11 27 53		

The pendulum, during this interval, vibrated about 1° 52′ each way from the perpendicular.

The tranſits of the ſtars over the meridian were obſerved with a teleſcope, fixt at right angles to an horizontal axis whoſe ends lay exactly eaſt and weſt; by the turning of which axis, the line of collimation of the teleſcope was conſtantly directed in the plane of the meridian. This inſtrument was daily adjuſted to a mark, fixt in the meridian; and in the journal, between the ſecond and third of *February*, the following remark was made.

N. B. This day was hotter than uſual, as appears by the thermometer; and the tranſit inſtrument had loſt the level a little; but after we had adjuſted it, it pointed exactly to our meridian mark; and therefore we are at a loſs for the cauſe of this difference in the clock.

From the foregoing table it appears, that the clock loſt 54″ 21‴ in 26 revolutions of the ſtars; that is, about 2′ 5″ and in one revolution; the difference from this medium ſomewhat varying upon account of a greater or leſs degree of heat on different days.

The mean of all the obſerved heights of the thermometer from *Jan.* 26, to *Feb.* 18, was about 12 diviſions and ½. Therefore, the difference between the mean heights of the thermometer at *Jamaica* and *London*, during the intervals of the reſpective obſervations, was 15 diviſions and ½; the ſpirit ſtand

ſtanding ſo much higher in *Jamaica*, becauſe of the greater heat in that iſland.

That we might be able to judge, how much the different degrees of heat, correſponding to any number of diviſions upon this thermometer would cauſe the clock to go ſlower, by lengthening its pendulum, Mr. *Graham* took notice of the loweſt point, to which the ſpirits ſunk at *London* in the winter 1731, and the greateſt height, to which they roſe in the following ſummer; and comparing the motion of the ſpirits in this thermometer, with the alterations in another made with quickſilver, which he had for ſome years made uſe of; he concluded that at *London* the ſpirits in this thermometer would ſtand (one year with another) about 60 diviſions higher in ſummer than in winter.

By ſeveral years experience he likewiſe found, that his clocks (of the ſame ſort with Mr. *Campbell*'s) when expos'd, as uſual, to the different degrees of heat and cold of our climate, do not vary in their motion above 25 or 30 ſeconds in a day.

From theſe obſervations and experiments we may, therefore, reaſonably conclude, that ſufficient allowance will be made for the lengthening of the pendulum by heat, if we ſuppoſe the clock upon that account to go one ſecond in a day ſlower, when the ſpirits of this thermometer ſtand two diviſions higher, and in the ſame proportion for other heights.

Admitting then that the mean height of the thermometer, while the clock was compar'd with the ſtars at *Jamaica*, exceeded that at *London* between 15 and 20 diviſions; if we allow eight or nine ſeconds, upon that account, the remaining difference muſt be entirely owing to the difference of the force of gravity in the two places.

Upon comparing the obſervations, it appears, that in one apparent revolution of the ſtars, the clock went 2′ 6″ and $\frac{1}{2}$ ſlower in *Jamaica* than at *London*; deducting, therefore, 8″ and $\frac{1}{2}$, on account of the greater heat in *Jamaica*, there remains a difference of 1′ 58″, which muſt neceſſarily ariſe from the diminution of gravity, in the place neareſt the equator.

Mr. *Bradley* has allow'd the clock to have loſt ſomewhat more on account of the difference of heat than the mean heights of the thermometer may ſeem to require, upon a ſuppoſition, that the total heat of the days, compared with the cold of the nights, bears a greater proportion in *Jamaica*

than *London*; but if that ſuppoſition be not admitted; then the clock in *Jamaica* muſt have gone rather more than 1′ 58″ in a day ſlower than in *England*.

Mr. *Campbell*'s obſervations were made at *Black-river* in 18° N. Lat. Now if we ſuppoſe with Sir *Iſaac Newton*, that the difference in the going of the clock is owing to the greater elevation of the parts of the earth towards the equator, it will follow from theſe obſervations, and what is deliver'd by him in Prop. 20. of the third book of his *Principia*, that the equatorial diameter is to the polar as 190 to 189; the difference between them being 41 miles and ½; which is ſomewhat greater than what Sir *Iſaac Newton* had computed from his theory, upon the ſuppoſition of an uniform denſity in all the parts of the earth.

Without entering into the diſpute about the figure of the earth, Mr. *Bradley* at preſent ſuppoſes with Sir *Iſaac Newton*, that the increaſe of gravity, as we recede from the equator, is nearly as the ſquare of the ſine of the latitude, and that the difference in the length of pendulums is proportional to the augmentation or diminution of gravity. Upon theſe ſuppoſitions Mr. *Bradley* collects from the above-mentioned obſervations, that if the length of a ſimple pendulum (that ſwings ſeconds at *London*, be 39.126 *Engliſh* inches, the length of one at the equator would be 39.00, and at the poles 39.206. And (abſtracting from the alteration on account of different degrees of heat) a pendulum clock, that would go true time under the equator, will gain 3′ 48″ and ¼ in a day at the poles: But the number of ſeconds, which it would gain in any other latitude, would be to 3′ 48″ and ¼ nearly, as the ſquare of the ſine of that latitude, to the ſquare of the radius: Whence it follows, that the number of ſeconds a clock will loſe in a day, upon its removal to a place nearer the equator, will be to 3′ 48″ and ¼ nearly, as the difference between the ſquares of the ſines of the latitudes of the two places, to the ſquare of the radius. Thus the difference of the ſquares of the ſines of 51° and ½ and 18°, the latitudes of *London* and *Black-river*, being to the ſquare of the radius, as 118 to 228 and ¼, the clock will go 1′ 58″ in a day ſlower at *Black-river* than at *London*, as was found by obſervation.

It may be hoped, that Mr. *Campbell*'s ſucceſs in this experiment, and the little trouble there is in making it, will induce thoſe gentlemen who may hereafter carry pendulum clocks

clocks into distant countries, to attempt a repetition of it after his manner; that is, by keeping or restoring the pendulum's of their clocks to the same length in the different places, and carefully comparing them with the heavens; and at the same time taking notice of the different degrees of heat, by means of a thermometer. From a variety of such experiments we should be enabled to determine how far Sir *Isaac Newton*'s theory is conformable to truth, with much greater certainty than from those trials which are made by actually measuring the lengths of simple pendulum's; because a difference of one hundredth part of an inch, in the length of a pendulum, corresponds to 11 seconds in a day: And it being easy to observe how much a clock gains or loses in a day, even to a single second. It is certain, that by means of a clock, compared in the manner abovementioned, we may distinguish a difference (in the lengths of isochronal pendulum's) of one thousandth part of an inch or less; whereas it will be scarce possible to measure their true lengths, without being liable to a greater error than that. Besides, by taking notice how much a clock gains or loses, upon the falling or rising of a thermometer, we can better allow for the different degrees of heat in this, than in the other method of making the experiment, by actual measurement; since it may not be easy to determine how much the measure itself, which we make use of, will be lengthened by different degrees of heat.

For these reasons Mr. *Bradley* reckons Mr. *Campbell*'s experiment to be the most accurate of all that have hitherto been made, and propereſt to determine the difference of the gravity of bodies in different latitudes; and therefore he subjoins a table he computed from it, that contains the difference of the length of a simple pendulum, swinging seconds at the equator, and at every fifth degree of latitude, together with the number of seconds that a clock would gain in a day, in those several latitudes, supposing it went true when under the equator; by means of which any one may readily compare other the like observations with his; and thereby discover whether the alteration of gravity in all places be uniform, and agreeable to the rule laid down by Sir *Isaac Newton* or not.

The

The Lat. of the Place.	The difference of the length of the pendulum in parts of an *Engliſh* inch,	Seconds gained by a clock in 1 day.	The Lat. of the place.	The difference of the length of the pendulum in parts of an *Engliſh* inch.	Seconds gained by a clock in 1 day.
Deg.	Inch.	Seconds	Deg.	Inch.	Seconds
5	0.0016	1.7	50	0.1212	134.0
10	0.0062	6.9	55	0.1386	153.2
15	0.0138	15.3	60	0.1549	171.2
20	0.0246	26.7	65	0.1696	187.5
25	0.0369	40.8	70	0.1824	201.6
30	0.0516	57.1	75	0.1927	213.0
35	0.0679	75.1	80	0.2003	221.4
40	0.0853	94.3	85	0.2050	226.5
45	0.1033	114.1	90	0.2065	228.3

Conjectures on the charming or faſcinating Power of the Rattle-ſnake, *grounded on credible Accounts, Experiments and Obſervations; by Sir* Hans Sloane. Phil. Tranſ. N° 433. p. 321.

AS to rattle-ſnakes, it is univerſally agreed on, that by keeping their eyes fixed on any ſmall animal, as a ſquirrel, bird or the like, tho' ſitting on the branch of a tree at a conſiderable height, it ſhall, by ſuch ſtedfaſt or earneſt looking thereon, be made to fall dead into their mouths.

Sir *Hans* had a rattle-ſnake given him, which had been ſent alive in a box with ſome gravel from *Virginia*; it had liv'd three months without any ſuſtenance and had in that time caſt its outer coat or *exuviæ*, which was found amongſt the gravel.

Captain *Hall*, a very underſtanding and obſervant perſon, who had liv'd ſeveral years in *Virginia*, ventur'd to take the ſnake out of the box; notwithſtanding that the poiſon from its bite is almoſt preſent death: For, he gave an inſtance of a perſon bitten, who was found dead at the return of a meſſenger going to the next houſe to fetch a remedy, tho' he was not gone above half an hour: Nay, ſo certain are the mortal effects of this poiſon, that ſometimes the waiting till an iron can be heated, in order to burn the wound, is ſaid to have prov'd fatal: He therefore thought the ſafeſt way was immediately to cut out the part where the wound was made:

For,

For, he had ſeen ſeveral, who carried theſe hollow ſcars about them, as marks of the narrow eſcape they had had, and they never felt any inconvenience afterwards.

Tho' providence hath produced a creature ſo terrible to other animals; yet it ſeems to have provided it with the rattle at its tail, that the noiſe thereof may give them warning to get out of its way.

An experiment was tried before ſeveral phyſicians in the garden belonging to the College in *London*: The Captain, by keeping the head faſt with a forked ſtick, and making a nooſe, which he put about the tail of the ſnake, tied it faſt to the end of another ſtick, with which he took the ſnake out and laid him upon the graſs-plat. Then a dog being made to tread upon him, he bit the dog, who thereupon howl'd very bitterly, and went away ſome few yards from the ſnake: But in about a minute of time he grew paralytic in the hinder legs, as dogs do who have the *aorta deſcendens* tied: He died in leſs than three minutes, as is related by Mr. *Ranby* in an account of this experiment in Phil. Tranſ. N° 401. p. 377. and by Captain *Hall* N° 399. p. 309.

According to Sir *Hans*, the whole myſtery of their inchanting or charming any creature, is chiefly this, namely, that when ſuch animals as are their proper prey, as ſmall quadrupeds, and birds, *&c.* are ſurpriſed by them, they bite them, and the poiſon allows them time to run a ſmall way, as the dog in the above experiment did; or perhaps a bird to fly up into the next tree, where the ſnakes watch them with great earneſtneſs, till they fall down, or are perfectly dead, when having lick'd them over with their ſpawl or ſpittle, they ſwallow them down, as the following accounts relate.

'Some people in *England (ſays Colonel* Beverly *in his* '*Hiſtory of* Virginia Edit. 2d. p. 260. Lond. 1722. 8vo.) are 'ſtartled at the very name of the rattle-ſnake, and fancy 'every corner of that province ſo much peſter'd with them, 'that a man goes in conſtant danger of his life, that walks 'abroad in the woods, But this is a groſs miſtake: For, 'firſt, this ſnake is very rarely ſeen, and when that happens, 'it never does the leaſt miſchief, unleſs you offer to diſturb 'it, and thereby provoke it to bite in its own defence: And 'it never fails to give you fair warning by making a noiſe 'with its rattle, which may be heard at a convenient diſtance. 'For my own part, I have travell'd the country as much as

'any

'any man; and yet before the firſt impreſſion of this book, 'I had never ſeen a rattle-ſnake alive and at liberty in all my 'life. The bite of this viper, without ſome immediate 'application, is certain death: But remedies are ſo well 'known, that none of their ſervants are ignorant of them. I 'never knew any one kill'd by theſe or any other of their 'ſnakes. They have ſeveral other ſnakes, which are ſeen 'more frequently, and have very little or no hurt in them; 'ſuch as the black-ſnakes, water-ſnakes and corn-ſnakes. The 'black viper-ſnake, and the copper-bellied ſnake, are ſaid 'to be as venemous as the rattle ſnake; but they are as 'ſeldom ſeen. Theſe three poiſonous ſnakes bring forth 'their young alive; whereas the other three ſorts lay eggs, 'which are afterwards hatch'd; and that is the diſtinction 'they make eſteeming thoſe only to be venemous, which 'are viviparous. They have likewiſe the horn ſnake, ſo 'call'd from a ſharp horn it carries in its tail, with which it 'aſſaults any thing that offends it, with ſuch force, that, as 'it is ſaid, it will ſtrike its tail into the but-end of a muſ-'ket, from which it is not able to diſengage itſelf.

'All ſorts of ſnakes will charm both birds and ſquirrels; 'and the *Indians* pretend to charm them. Several perſons 'have ſeen ſquirrels run down a tree directly into a ſnake's 'mouth: They have likewiſe ſeen birds fluttering up and 'down, and chattering at theſe ſnakes, till at laſt they have 'dropt down before them.

'In the latter end of *May*, 1715, ſtopping at an orchard, 'by the road ſide (being three of us in company) we were 'entertain'd with the whole proceſs of a charm between a 'rattle ſnake and a hare; the hare being better than half 'grown. It happened thus; One of the company, in his 'ſearch for the beſt cherries, eſpied the hare ſitting, and tho' 'he went cloſe by her, ſhe did not move; till he (not ſuſ-'pecting the occaſion of her tameneſs) gave her a laſh 'with his whip. This made her run about ten foot, and 'there ſit down again. The Gentleman not finding the 'cherries ripe, immediately return'd the ſame way; and near 'the place where he ſtruck the hare, he eſpied a rattle-'ſnake. And ſtill not ſuſpecting the charm, he goes back 'about 20 yards to a hedge to get a ſtick to kill the ſnake; 'and at his return found the ſnake remov'd and coil'd in the 'ſame place, from whence he had mov'd the hare. This 'put him into the thoughts of looking for the hare again,

and

'nd he ſoon ſpied her about 10 foot off the ſnake, in the 'ſame place to which ſhe had ſtarted when he whipt her. 'She was now lying down; but ſhe would ſometimes raiſe 'herſelf on her fore-feet, ſtruggling as it were for life, or to get 'away, but could never raiſe her hinder parts from the 'ground; and then ſhe would fall flat on her ſide again, 'panting vehemently. In this condition the hare and ſnake 'were when he call'd me; and tho' we all three came up 'within 15 foot of the ſnake, to have a full view of the 'whole, the ſnake took no notice at all of us, nor ſo much 'as gave a glance towards us. There we ſtood at leaſt half 'an hour, the ſnake not altering a jot; but the hare often 'ſtruggling and falling on its ſides again, till at laſt the hare 'lay ſtill for ſome time as dead. Then the ſnake mov'd out 'of his coil, and ſlid gently and ſmoothly on towards the 'hare, his colours at that inſtant being ten times more glo-'rious and ſhining than at other times. As the ſnake mov'd 'along, the hare happened to fetch another ſtruggle, upon 'which the ſnake made a ſtop, lying at his length, till the 'hare had lain quiet again for a ſhort ſpace; and then he 'advanc'd again, till he came up to the hinder parts of the 'hare, which in all this operation had been towards the 'ſnake. There he ſurveyed the hare all over, raiſing part 'of his body above it, then he turned off, and went to the 'head and noſe, after that to the ears; took the ears in his 'mouth one after the other, working each apart in his mouth, 'as a man does a wafer to moiſten it; then return'd to the 'noſe again, and took the face into his mouth, ſtraining and 'gathering his lips, ſometimes by one ſide of his mouth, 'ſometimes by the other. At the ſhoulders he was a long 'time puzzled, often haling and ſtretching the hare out at 'length, and ſtraining forward firſt one ſide of his mouth, 'then the other; till at laſt he got the whole body into his 'throat. Then we went to him, and taking the twiſt-band 'off from my hat, I made a nooſe, and put it about its 'neck. This made him at length very furious: But having 'ſecured him, we put him into one end of a wallet, and 'carried him on horſeback five miles to the houſe where we 'lodged that night. Next morning we killed him and took 'the hare out of his belly. The head of the hare began to 'be digeſted, and the hair to fall off, having lain about 18 'hours in the ſnake's belly.

'In my youth I was a bear-hunting in the woods, above the 'inhabitants; and having ſtrayed from my companions, I was 'entertain'd upon my return with a relation of a pleaſant ren-'countre between a dog and a rattle-ſnake, about a ſquirrel. 'The ſnake had got the head and ſhoulders of the ſquirrel into 'his mouth, which being ſomething too large for his throat, it 'took him up ſome time to moiſten the furr of the ſquirrel 'with his ſpawl, to make it ſlip down. The dog took this 'advantage, ſeized the hinder parts of the ſquirrel, and tugged 'with all his might. The ſnake on the other ſide would not let 'go his hold for a long time; till at laſt, fearing he might be 'bruis'd by the dogs running away with him, he gave up his 'prey to the dog. The dog eat the ſquirrel and felt no harm.

'Another curioſity concerning this viper, I will relate from 'my own obſervation. A rattle-ſnake being taken in a nooſe, 'I cut off the head, leaving about an inch of the neck with it: 'This I laid upon the head of a tobacco hogſhead, one *Stephen* '*Lankford*, a carpenter, being with me. Now theſe ſnakes 'have but 2 teeth, by which they convey their poiſon, and 'theſe are placed in the upper jaw, pretty forward in the mouth, 'one on each ſide. Theſe teeth are hollow and crooked like a 'cock's ſpur: They are alſo looſe or ſpringing in the mouth, 'and not faſtened in the jaw-bone, as all the other teeth are. 'The hollow has alſo a vent thro' by a ſmall hole a little be-'low the point of the tooth. Theſe 2 teeth are kept lying 'down along the jaw, or ſhut like a ſpring knife: They have 'alſo over them a looſe thin film or ſkin of a fleſh colour, which 'riſes over them when they are rais'd. This ſkin does not 'break by the riſing of the tooth only, but keeps whole till the 'bite is given, and then it is pierced by the tooth, by which 'the poiſon is let out. The head being laid upon the hogs-'head, I took two little twigs or ſplinters of ſticks; and having 'turned the head upon its crown, open'd the mouth, and lifted 'up the fang or ſpringing tooth on one ſide ſeveral times; in 'doing of which I at laſt broke the ſkin. The head gave a 'ſudden champ with its mouth, breaking from my ſticks; in 'which I obſerved that the poiſon ran down in a lump like oil, 'round the root of the tooth. Then I turned the other ſide of 'the head, and reſolv'd to be more careful to keep the mouth 'open on the like occaſion, and obſerve more narrowly the con-'ſequence: For, it is to be obſerv'd, that tho' the heads of 'ſnakes, terrapins (*a ſort of tortoiſe*) and ſuch like vermin be 'cut off; yet the body will not die in a long time after, the

'general

'general saying is, not till the sun sets. After opening the 'mouth on the other side, and lifting up that fang also several 'times, he endeavour'd to give another bite or champ. But I 'kept his mouth open, and the tooth pierced the film, and 'emitted a stream like that in blood-letting, and cast some drops 'upon the sleeve of the carpenter's shirt, who had no waist-'coat on. I advis'd him to pull off his shirt, but he would 'not, and receiv'd no harm; and tho' nothing could then be 'seen of it upon the shirt; yet in washing there appear'd 5 'green specks, which every washing appear'd plainer and 'plainer, and lasted as long as the shirt, which was about 3 'years after. The head we afterwards threw down upon the 'ground; and a sow came and eat it before our faces, and re-'ceiv'd no harm. Now I believe, had this poison lighted upon 'any place of the carpenter's skin, that was scratch'd or hurt, it 'might have poison'd him. I take the poison to rest in a small 'bag or receptacle in the hollow at the root of these teeth; but 'I never had the opportunity afterwards to make a farther dis-'covery of that.

'As to the violent effects of this poison I was told by Colonel '*James Taylor*, that being with others in the woods a survey-'ing, they found a rattle-snake, and cut off his head, and about '3 inches of the body. Then with a green stick which he had 'in his hand, about a foot and a half long, the bark being 'newly peel'd off, he urged and provoked the head; till it bit 'the stick with fury several times. Upon this the Colonel ob-'served small green streaks to rise up along the stick towards 'his hand. He threw the stick upon the ground, and in a quar-'ter of an hour, the stick of its own accord split into several 'pieces, and fell asunder from end to end. This account I had 'from him again at the writing hereof.'

F. *La Bat* likewise tells us (in his *nouveau voyage aux isles de l'Amerique* Tom. 4. p. 96, 106. *Edit. Paris*, 1722, 8°) that serpents, when they bite their prey, retire to avoid being hurt by them; and when dead, cover them with their spittle, extend their feet along their sides and tails, if quadrupeds, and swallow them.

The Squilla aquæ dulcis; *by Dr.* Richardson. Phil. Trans. N° 433. p. 331.

DR. *Richardson* observ'd what he does not remember to be taken notice of by any naturalist; namely, the great destruction made amongst the small fry of fish by the *squilla*

aquæ dulcis, which abound in moſt ſtanding waters. In a ſmall breeding pond near his houſe, where he had formerly plenty of ſmall carp and tench every year, but more latterly ſcarce any young fry to be met with, his gardener obſerved one of the *ſquillæ* with a carp in its mouth almoſt as big as itſelf; and he afterwards obſerved theſe inſects hunting amongſt the weeds, and vigorouſly purſuing the ſmall fry. The Dr. order'd the gardener to catch ſome of theſe inſects, and bring them home alive, with ſome of the ſmalleſt fiſh he could meet with. They were put together in a large baſon of water; and the inſects were ſo rapacious, that they fell upon the fiſh immediately, and deſtroy'd ſeveral in his ſight; and before morning they devour'd all that were in the baſon.

A ſolar Eclipſe *obſerved at* Witemberg *a little before ſun-ſet*, May 2. 1733, O. S. *by M.* Weidler. Phil. Tranſ. N° 433. p. 332. *Tranſlated from the* Latin.

Phaſes of the immerſion

	H.	′	S.	
May 2, 1733, O. S. The beginning of the eclipſe at — —	6	36	5	*p. m.*
1 digit eclipſed —		39	50	
2 digits —		45	5	
3 — —		48	50	
4 — —		52	50	
5 — —		58	5	
6 — —	7	2	50	
7 — —		7	50	
8 — —		10	50	
9 — —		15	50	
10 — —		19	50	
11 — —		29	20	

The ſun goes under thin clouds.

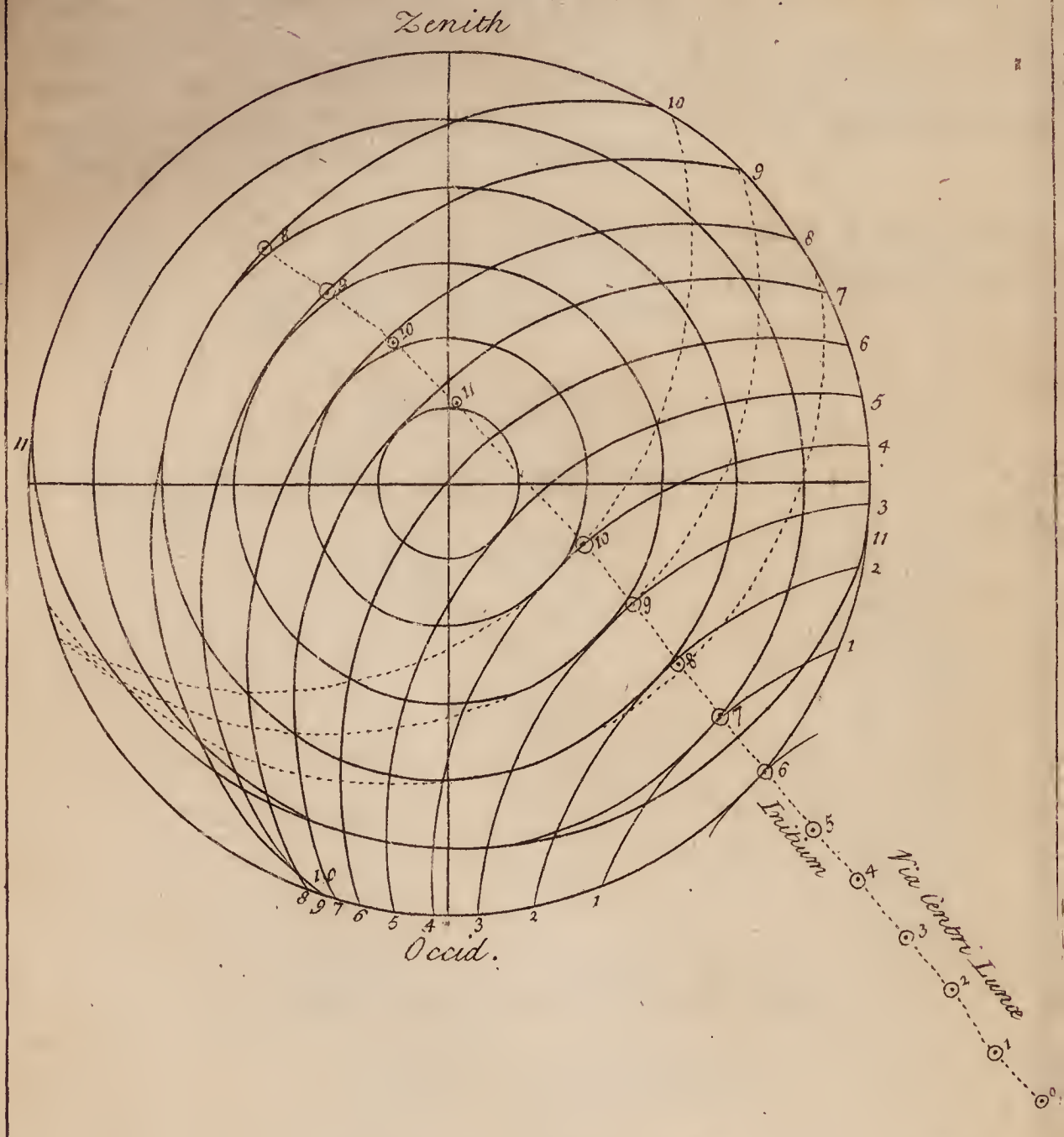
Phaſes of the emerſion.

10 digits eclips'd	7	35	50
9 —		40	50
8 —		44	50
The ſun ſets at — —		46	5

M. *Weidler* makes the following remarks.

1. Thi

Fig. I.



1. The circle (Fig. 1. Plate I.) repreſents the image of the ſun, as it appear'd in the bottom of the helioſcope.

2. The light of the ſun, near the moon's diſk, which M. *Weidler* had always obſerv'd, in other ſolar eclipſes, as well near the horizon, as under a greater altitude, to have a ſtrong undulatory motion, was quite ſtill and calm in this.

3. The moon's diſk, eſpecially on its weſtern part, in the phaſes obſerv'd a little before ſun-ſet, appear'd to all the beholders to have a manifeſt aſperity; yet there were ſpaces, neither very broad nor deep, by which the tops of the lunar mountains were diſtinguiſhed: And with a nicely divided ſcale he eſtimated the depth of one valley to be $\frac{1}{200}$ part of the moon's diameter.

4. The laſt phaſes of the emerſion were ſeen thro' thin clouds; and yet the moon did not cover above 11 dig. of the ſun's diſk.

5. The ſetting of the ſun's centre, for the horizon of *Witemberg* was found to be at $7^h\ 39'\ 49''$; and conſequently, by the refraction of the rays in the clouds in the horizon it was retarded 6 min. of time nearly.

An Abſtract of Meteorological Diaries; *with Remarks thereon; by Dr.* Derham. Phil. Tranſ. N° 433. p. 334.

THE following meteorological obſervations were very carefully made in 1726 (ſome twice and ſome thrice every day) at *Berlin*, by the Society there; at *Lunden* in *Sweden* by *Conrad Quenſel*, Profeſſor of the Mathematics in the *Caroline* Academy; at *Bettna* in *Sudermanland*, by *Andr. Geringius*, Paſtor and Provoſt of the place; at *Upſal* by *Eric Burman*, Profeſſor of Aſtronomy in the *Guſtavian* Academy; at *Bygdea* in *Weſtro-Bothnia* by *Jacob. Burman*, Paſtor of the place; and at *Pythea* in the ſame province, by *Olave Burman* and *Iſrael Steckſenius*, ſtudents.

The moſt uſeful of the barometrical obſervations are exhibited in the following table, which readily ſhews the higheſt, loweſt, and mean, heights of the quickſilver in every month, at the ſeveral places.

	JANUARY.			
	Berlin	Lun.	Bett.	Upſa.
High.	29.3	30.0$\frac{1}{2}$	30.51	30.18
Mean	28.7$\frac{1}{2}$	29.3$\frac{1}{8}$	29.92	29.58
Low	28.0$\frac{1}{4}$	28.5$\frac{5}{4}$	29.26	28.98

FEBRUARY.

	Berlin	Lun.	Bett.	Upſa.
High.	29.6	29.8¾	30.40	30. 2
Mean	28.6¼	29.1½	29.74	29.53
Low.	27.6¾	28.4¼	29. 8	28.86

MARCH.

	Berlin	Lun.	Bettna	Upſa.	Pithea
High.	29. 0¾	30.0½	30.50	30.24	30.11
Mean	28. 5	29.4½	29.79	29.28	29.35
Low.	27.10½	28.8½	29. 8	28.32	28.50

APRIL.

	Berlin	Lun.	Bettna.	Upſal	Pithea
High.	28.10	29.9½	30.48	30.17	29.98
Mean	28. 5	29.5⅞	29.86½	29.66	29.27½
Low.	28. 0⅕	29.2¼	29.25	29.15	28.57

MAY.

	Berlin	Lun.	Bettna	Upſa.	Pithea
High.	28. 9¼	30.0½	30.40	30.16	30.11
Mean	28. 4⅕	29.7⅜	30.77½	29.84	29.74½
Low.	28.00 ½	29 4¼	29.35	29.52	29.48

JUNE.

	Berlin.	Lun.	Bettna	Upſal	Pithea
High.	28.7½	29.9½	30.20	30.00	29.98
Mean	27.9¼	29.4½	29.67½	29.62½	29.56½
Low.	27.0½	28.9½	29.15	29.25	29.15

JULY.

	Berlin	Lun.	Bettna	Upſa.	Pithea
High.	28.5⅘	29.6	30. 5	29.88	29.56
Mean	28.2¾	29.2¾	29.70	29.54	29.37½
Low.	28.0¼	28.9½	29.35	29.20	29.19

AUGUST.

	Berlin.	Lun.	Bettna	Upſal	Pithea
High.	28. 8	29.8¾	30.30	29.98	29.86
Mean	28. 3¼	29.3⅝	29.65½	29.48	29.28½
Low.	27.11⅘	28.8½	29. 1	28.98	28.71

SEP-

SEPTEMBER.

	Berlin	Lun.	Bettna	Upſal	Pithea
High.	28.6	29.7¾	30.28	30.00	29.80
Mean	28.1	29.1¾	29.57	29.29	29.20
Low.	27.8	28.5¾	28.87	28.58	28.60

OCTOBER.

	Berlin	Lun.	Bettna	Upſal	Pithea
High.	28.10	30.1½	30.55	30.25	29.90
Mean	28. 3½	29.1⅞	29.57½	29.28½	29.05
Low.	27. 9	28.2¼	28.60	28.32	28.20

NOVEMBER.

	Berlin	Lun.	Bettna	Upſal	Pithea
High.	29.1	30.1½	30.80	30.51	30.19
Mean	28.7	29.5½	30.00	29.73	29.24
Low.	28.1¼	28.9½	29.20	28.90	28.29

DECEMBER.

	Berlin	Lun.	Bettna	Upſal	Pithea
High.	29.1⅔	30.0½	30.50	30. 7	29.80
Mean	28.4¾	29.5½	29.65	28.83½	29. 0½
Low.	27.8¼	28.4¼	28.80	28.60	28.21

Tho' this table may give a good view of the barometrical ranges at the ſeveral places in every month of the year 1726; yet Dr. *Derham* thinks it neceſſary to take notice of the great agreement between the aſcents and deſcents of the mercury, ſometimes at the very ſame time, and generally near it. If the mercury was remarkably high or low, it was ſo in all or moſt of the places: If ſtationary for 3 or 4, or more days, it was the ſame in all; only the alteration would begin or end, ſomewhat ſooner or later, perhaps in one place than the other; and when there was any deviation from this rule, it was commonly moſt remarkable in the *Pithea* obſervations.

As to the thermometrical obſervations, the Dr. could give no account of them, by reaſon he did not underſtand the thermometers made uſe of, nor the freezing, temperate, or other points; only the *Upſal* thermometer (which was made by Mr. *Hauksbee*) muſt ſerve for all; in which the point of extreme heat is marked 5 degrees above 0, and ſo is graduated downwards to 45 degrees, which is the point of temperate, and 65 degrees,

degrees, which is the point of freezing. The mean of all th degrees of every month at *Upſal* M. *Burman* hath noted, ac cording to Dr. *Jurin*'s directions in *Phil. Tranſ.* N° 379 which is, by adding the whole degrees of the month, and di viding by the number of days; which mean degrees Dr. *Des ham* has inſerted, as well as his own; which laſt are the mean between the higheſt and loweſt degrees, as well of the thermo meter as barometer.

A table of the mean barometrical ſtations by Dr. *Jurin* way.

JANUARY.		FEBRUARY.		MARCH.	
Upſale	Pithea	Upſale	Pithea	Upſale	Pithea
29.76 $\frac{10}{31}$		29.47 $\frac{11}{14}$		29.51 $\frac{3}{31}$	29.28 $\frac{15}{31}$
APRIL.		MAY.		JUNE.	
Upſale	Pithea	Upſale	Pithea	Upſale	Pithea
29.76 $\frac{2}{3}$	29.49 $\frac{7}{15}$	29.91 $\frac{15}{31}$	29.82 $\frac{22}{31}$	29.59 $\frac{11}{15}$	29.49 $\frac{3}{5}$
JULY.		AUGUST.		SEPTEMBER.	
29.53 $\frac{11}{31}$	29.38 $\frac{3}{19}$	29.54 $\frac{17}{31}$	29.27 $\frac{12}{31}$	29.34 $\frac{9}{10}$	29.14 $\frac{1}{6}$
OCTOBER.		NOVEMBER.		DECEMBER.	
29.43 $\frac{13}{31}$	29.11 $\frac{17}{30}$	29.81 $\frac{1}{11}$	29.49 $\frac{23}{30}$	29.66 $\frac{23}{31}$	29.19 $\frac{9}{31}$

A thermometrical table of the higheſt, loweſt, and mea ſtations at *Lunden* and *Upſal*; together with the mean ſtation at *Upſal* according to Dr. *Jurin*'s method.

	JANUARY.			FEBRUARY.			MARCH.		
	Lun.	Upſa.	Upſale	Lun.	Upſale		Lun.	Upſale	
High.	79	96.		85	83.7		111	82.0	
Mean	58 $\frac{1}{3}$	74.8	81.2 $\frac{13}{31}$	65 $\frac{1}{2}$	74.3 $\frac{1}{2}$	74.1 $\frac{21}{31}$	84 $\frac{1}{2}$	67.5	69..
Low.	38	67.7		56	65		58	52.4	
	APRIL.			MAY.			JUNE.		
High.	149	60. 9		187	47. 8		188	46. 7	
Mean	116	56.56	53.7 $\frac{17}{31}$	156	32.54	33.0 $\frac{2}{31}$	158 $\frac{1}{2}$	30.55	34..
Low.	83	43. 3		125	18. 1		129	15. 4	

Ju

	JULY.			AUGUST.			SEPTEMBER.		
	Lun.	Upſa.	Upſale.	Lun.	Upſa.	Upſale.	Lun.	Upſa.	Upſale
igh.	173	42. 4		156	52. 2		168	62.7	
lean.	146	32.54	33·7$\frac{15}{30}$	134$\frac{1}{2}$	41.60	43·5$\frac{13}{31}$	133	50.4	43·5$\frac{1}{31}$
ow.	119	23. 4		113	32. 8		98	38.1	

	OCTOBER.			NOVEMBER.			DECEMBER.		
igh.	122	70.7		90	84.3		83	94. 8	
lean.	102$\frac{1}{2}$	61.7	61.8$\frac{27}{31}$	71	73.0	72.12$\frac{1}{8}$	61	80.57	79.8 $\frac{5}{31}$
ow.	83	52.7		52	62.3		40	67. 7	

By this table, eſpecially by the *Upſal* obſervations, it appears, that the colder months in this year 1726, were not ſo exceſſive cold, as their northerly ſituation would incline one to imagine; *Upſal* itſelf being 60 degrees north. But by the table, ſome of the days in *January*, *February*, *March*, &c. at the beginning of the year; and of *September*, *October*, *November* and *December*, at the latter end of it, may be obſerv'd to have had the thermometer ſometimes not ſo low, or very little below the freezing point. In *January* and *December*, for inſtance, when it was at 67.7, which was lower than in the other months, it was not 5 degrees lower than the freezing point at 65 degrees.

But by the beſt judgment Dr. *Derham* could make of the thermometrical obſervations at *Berlin*, they ſeem to have had no leſs, if not more ſevere weather, than in the northern parts, particularly than at *Lunden*, *Upſal* and *Pithea*, where the weather ſeems to have been milder than at *Betna* and *Bygdea*; at which two places, the Dr. finds they had frequent ſhowers of rain and ſtorms of ſnow, and more hard weather than at the other *Swediſh* places, or at *Berlin*.

What the cauſe of this different warmth ſhould be, the Dr. leaves others to judge; whether the proximity of the ſea, or the warmth of mineral vapours, and their woods ſkreening off the cold winds; to which two latter *Olaus Magnus* aſcribes a great deal.

But for the better judging of the ſtate of every month, take the following view, which the curious author of the *Betna* obſervations hath given; together with ſome remarks of the Dr's own from the other places.

In *January* he ſays, the winter cold (which was very intenſe from the 23 of *December* to the 15 of *January*) began

to abate, to the diſadvantage of the roads and travelling. In *February* the winter weather continued all the month, to the middle of *March*, with ſome ſnow, and froſt enough to benefit the roads and travelling. *March* he ſays began with ſnow, ſtormy and bitter cold; but towards the latter end the weather was milder, and more ſeaſonable for agriculture, juſt begun. On the 14. and 17. there was an *aurora borealis*

On the 17. and 22. of *March* there were likewiſe ſigns of *auroræ boreales* at *Berlin*; as alſo on the 23. of *February*.

At *Upſal* there was alſo a *lumen boreale* on the 27. of *February*, the 3, 15, and 16 of *March*.

In *April*; *Betna* is ſaid to have a ſeaſonable ſeed-time and that the autumnal corn, which had eſcap'd the worm (a calamity common in thoſe parts as in *England)* began now to flouriſh.

At *Lunden* they had *parhelia* on the 28. and 29. of *April*.

At *Upminſter* we had the *aurora borealis*, or ſtreaming, on the 12. of *April* in the evening.

In *May* the obſerver at *Betna* takes notice, that by the continual and pernicious heat of the ſun in this month, the corn was burnt up in ſuch manner, as to be a ſad preſage of an enſuing ſcarcity and dearth of proviſions.

And likewiſe at *Upſal* and *Berlin* they mention great drought and exceſſive heat of the ſun. But in ſome parts of the month, the air at *Berlin* is ſaid to have been coldiſh. In *June* the violent heats were abated, and the ſeaſon was more moiſt and rainy. The corn being too ſoon ripe caus'd their harveſt to fall out at a very unuſual ſeaſon.

At *Lunden* and *Berlin* it was cold ſeveral days and irkſome.

July was a rainy month at *Berlin*, and in moſt of the *Swediſh* places; at *Pithea* the leaſt. At *Betna* it was very unwelcome to the harveſt people. There was alſo much thunder and lightning in moſt of the places, chiefly at *Upſal*.

In *Auguſt* the Dr. finds a greater agreement between the winds than in the other months; they, in moſt of the places blowing from ſome of the points between the W. and S. At *Berlin* and *Upſal* there was a deal of rain; at *Pithea* thunder, and at *Betna*, the beginning of the month, being mild and fair, is ſaid to be a good ſeed-time; but it is remarked that for want of rain the ſeed did not come up well.

Sept

September was a very rainy month in all the *Swediſh* places; very cloudy, and ſome miſty and ſnowy at *Pithea*; but at *Berlin* better weather. At *Lunden* there was a parhelion on the 11th of *Sept.*

In *October* the *Swediſh* places had ſeveral *auroræ boreales*. At *Lunden*, on the 8, 12 and 24. At *Betna*, on the 8, 10, 12, 13, 15, 22 and 26. At *Upſal*, on the 3, 6, and eſpecially the 8. And at *Upminſter*, the ſame evening of *October* 8, a very remarkable whitiſh liſt, or girdle, went croſs the heavens, from weſt by ſouth, to eaſt by north, about half a degree broad; which continued but a little while, and then the whole hemiſphere was cover'd with ſtreaming vapours, emitting lances every where that pointed towards the zenith, where they formed a canopy, ſometimes reddiſh, ſometimes darker, and ſometimes blazing, as if ſet on fire, and emitting lances every way, ſo as to make an appearance like the ſtar which the Knights of the Garter wear. This canopy mov'd ſometimes ſome degrees eaſtward, and then would return back again near the zenith. When the vapours and lances ſhone out moſt, the Dr. obſerv'd a ſtrange commotion and working in them, as if ſome large body was behind them and diſturbed them.

The weather in this month was rain and hoar-froſt in the *Swediſh* places, with a deal of ſnow at *Pithea* and *Bygdea*; a parhelion at *Lunden Oct.* 14: And the *Betna* obſerver ſaith, that the plenty of rain this month caus'd the corn to thrive much; and he reckons the 31ſt of this month to be then the firſt winter day with them; it being froſty, and abundance of ſnow. At *Berlin* it ſeems to have been a dark and cloudy month, with irkſome cold towards the latter end.

At *Lunden* there were *auroræ boreales* on the 2, 7 and 8 of *November*; at *Betna* the 2; at *Lunden* and *Upſal*, it was cloudy, foggy, froſt and ſnow; at *Bygdea*, *Pithea* and *Berlin*, fairer with froſt and ſevere cold. At *Betna* the cold was very intenſe; the heavens very cloudy and miſty.

In *December*, there were *auroræ boreales* at *Lunden* on the 5, 6, 7, 8, 9, 10, 14, 15, 16 and 22 days; and at *Upſal* on the 5 and 6; and at *Berlin* there were ſignals of the *auroræ boreales* on the 7 and 12; at *Lunden* froſt and ſnow, cloudy and fogs; at *Upſal* ſome cloudy and foggy, and ſometimes a pretty deal of fair; at *Berlin* much froſt, cold, and a great ſtorm of wind on the 23; at *Pithea* frequent ſnow, and ſome

fair, ſome cloudy. *Betna* had moderately ſnow, but twice more intenſe cold, to the great benefit of the roads and travelling.

For the better underſtanding the preceeding obſervations, it is to be obſerv'd, that the *Lunden* barometrical obſervations were made with a barometer, graduated according to *Engliſh* meaſure into inches, and the Dr. ſuppoſes, decimal parts of inches: But the thermometer he underſtood not: The *Betna* barometer is alſo graduated in the ſame manner into *Engliſh* inches and centeſimals. The *Upſal* barometer and thermometer were both made by Mr. *Hauksbee*, and conſequently are according to *Engliſh* meaſure, the barometer having inches and centeſimal parts, the thermometer as before deſcribed. The *Bygdea* obſervations had none thermometrical or barometrical, only a verbal account of the weather, and now and then of the winds. The *Pithea* obſervations had none thermometrical; and thoſe of the barometer ſeem to be in inches and centeſimal parts.

Of Ambergreaſe; *by Dr.* Neuman. Phil. Tranſ. N° 433. p. 344. *Tranſlated from the* Latin.

TWO things in phyſick Dr. *Neuman* has often been juſtly and very much ſurpriſed at; 1. That we have no certain accounts of ſome ſpecies, which have for a long time; nay, ſome of them for ſeveral ages, been uſed in pharmacy; ſo that in *Europe* alone, there is yearly conſumed ſeveral hundred weight of them; eſpecially of the various exotic vegetables, as myrrh, *gum animæ*, *carannæ*, *ſanguis draconis*, *ſemen cinæ*; and ſeveral other ſpecies in the other kingdoms; and yet as to their natural hiſtory, native ſoil, origin and manner of production, we are entirely at a loſs; and our accounts of ſome of them are doubtful, various, contradictory, and generally by hearſay only.

2. That few pharmaceutic ſubjects, tho' in uſe for ſo many ages back, are hitherto but little known, as to their true compoſition and conſtituent parts, and their peculiar and real nature; and that we are ignorant of the demonſtrable, ſeparable parts they may contain, not only the viſible, but chiefly the parts perceivable by the ſmell and taſte, and which are really active: And that we are at a loſs in what ſeparable ſubſtance, and part of the natural compound (tho' call'd a ſimple by the vulgar) the eſſential characteriſtic, which diſcovers itſelf by its ſpecific ſmell, taſte and virtue, does eminently reſide: Whereas the greateſt part of our

account

accounts is taken from travellers, merchants, druggiſts and politicians, who are generally unſkilled in medicine and natural hiſtory, and from jeſuits, monks, ſoldiers, ſailors, fiſhermen, miners, gardeners, nay even ſome phyſicians, who are not ſufficiently grounded in the experience of true chemiſtry and demonſtrative phyſic: And theſe accounts have been tranſcribed, and credited; and continue to be ſo ſtill; yet upon an impartial and unprejudiced examination; what has hitherto been ſo ſaid, written and credited, is found to be nothing leſs than the real truth, but rather the contrary. The blunders in phyſic as to theſe two circumſtances are ſcarce to be imagined, could not ſeveral inſtances be adduced to that purpoſe: The Dr. had for ſome years propoſed to himſelf, and at the ſame time taken no ſmall pains to examine carefully the common ſpecies; and the reſult was, that he diſcover'd many truths, and expoſed ſeveral falſe accounts and chimerical notions.

He indeed owns, that he met with inſurmountable obſtacles in a great many things relating to the firſt circumſtance, namely the natural hiſtory; partly, as he himſelf had not been in the native country of theſe exotic animals and vegetables; partly as the accounts of them in books are merely equivocal, uncertain and entirely diſagreeing with their preparations; and partly likewiſe becauſe in many, eſpecially, moſt of the mineral ſpecies, it is impoſſible to be preſent at their generation and manner of production, or ſo viſibly obſerve it, as we may probably to greater advantage in vegetables and animals; on which account, he ſo much the more ſeriouſly applied himſelf to the ſecond circumſtance, namely the inveſtigation of the compoſition of natural ſubſtances, that he might, at leaſt have a thorough knowledge, as to their natural compoſition, of the things commonly made uſe of in the ſhops, whether from the *Eaſt* or *Weſt Indies*, *Europe* or *Africa*; and whether they be vegetables, animals or minerals, or of whatever denomination they commonly are, before they are made into medicines or other preparations for common uſe: In this diſquiſition he eſpecially made uſe of a nice chemical analyſis, and that the rather, as he found the methods by microſcopes, geometrical figures, and by pneumatical, hydroſtatical and the other uſual ways of trial, not only manifeſtly inſufficient, but often fallacious; nay, of little ſervice in phyſico-chemical or demonſtrative diſquiſitions.

The ſubject of the Dr's preſent diſquiſition is ambergreaſe, commonly us'd in the ſhops; it has been much and long dif-

diſputed, what it properly is: And by ſome accounts of it latterly from *America*, it ſhould ſeem it was certainly diſcover'd, what it really is; but as theſe accounts do not agree with the chemical analyſis he made of this matter, he could not receive them as deciſive; and he was therefore induced to write the following account, in order to excite the enquirers into nature, both in *America* and elſewhere, to gain farther certainty in this affair.

Not to enter into an etymological diſquiſition of the term *ambra*; he thinks it ſufficient, that it is now commonly uſed, and its meaning univerſally underſtood: And not to mention the ſeveral ſorts of amber, it is obſervable what *Ulyſſes Aldrovandus* affirms in his *Muſæum Metallicum lib.* 3. *cap.* 21. p. 436; that under the general term *ambra* is underſtood the whole genus of amber; and that at this day ambergreaſe is call'd by ſome *ſuccinum orientale*, as for ſome time back, the common amber has uſually been call'd *ſuccinum occidentale*, to diſtinguiſh it from the oriental: But it is a thing well known, that among ſeveral *European* nations, the term *ambra* without any adjunct, denotes common amber; from which obſervation he is induced to think that the ancient *Pruſſians*, who uſed the term *ambra* to denote both common amber and ambergreaſe, had at that time a true idea of ambergreaſe, as to its nature or conſtituent parts.

He cannot but own, that conſidering the many various and different opinions that formerly have been, and ſtill are entertain'd about ambergreaſe, that produce of the ſea, he ſeems as if he would plunge into a wide ocean of opinions, and there from ſo many uncertain accounts and deſcriptions draw a certain concluſion: For, there are doubtleſs very few things in nature, to which ſo many different ways of formation have been aſcribed as to this ſubject; and not content with the three kingdoms, they have deviſed two more, in which they have claſſed it, as the aerial and marine; tho' the whole ſea with all its contents be reducible to any of the three common kingdoms of nature, the mineral, vegetable or animal.

Oelven is the only one, as far as the Dr. knew, who claſſed ambergreaſe in the aerial kingdom, taking it for a meteor, or ſomething generated in the air; and this manifeſtly abſurd opinion he endeavours to ſupport by ſeveral reaſons, which notwithſtanding are of no great weight; beſides, that it is ſufficiently confuted from the weight of ambergreaſe, which ſometimes amounts to 100 pounds, and from its ſubſtance, and the compoſition of its eſſential parts. Many

Many have recourſe to the animal kingdom, yet without entirely quitting the aerial: for, they hold that ambergreaſe derives its origin from ſuch creatures as fly in the air, tho' this again be with a remarkable difference, which divides them into two claſſes; the firſt ſuppoſes ambergreaſe to be the dung of birds, and they are led into this opinion from the becks, claws, and other parts of theſe creatures being found therein: nay, they deſcribe the very bird, affirming it to be as big as a gooſe, with beautiful and ſpotted plumes, and in the *Maldivian* language call'd *Anacangripaſqui*, and in that of *Madagaſcar*, *Aſchibobuck*. *Ferdinandus Lopez* de *Caſtagneda*, lib. 4. cap. 35. and others are of opinion, that this bird eats ſeveral fragrant herbs, and depoſits its precious dung upon the rocks in and about the ſea; which dung after being digeſted and concocted by the ſun in the day time, and depurated by the moon in the night, and conſequently ripened, prepared, and perfected by both luminaries into ambergreaſe; and afterwards in ſtormy weather waſh'd off by the waves, is carried floating into the ſea; and at length, either driven aſhore, ſuch as it was waſh'd from the rocks, or ſwallowed by whales; who, being unable to digeſt it, do generally throw it up again in a ſhort time.

The other opinion on ambergreaſe is, that it is produced by bees, and that it is their wax; of this opinion chiefly is Denis, *Rec. des Mem. & Conferen. ſur les Arts & les Sciences* 1672. *M. Aug. p.* 222. *It. Mediſeri Coſmograph. Tom. I. p.* 101. *It. Ejus deſcriptio inſulæ Madagaſcar, cap.* 6. *p.* 43. *It. Odoardus Barboſa. It. Andr. Tevet. and Franc. Belloforeſtus.* Monconys *in Itinerar. ſuo Edit. Pariſ. en ſuite de la* 2. *partie, p.* 143. *It. Edit. Londinenſ. p.* 71. *Lemery, traitè univerſ. des drogues ſimples.* And. Pomet, *hiſt. generale des drogues, P.* 2. *lib.* 1. *p.* 57. if not the greateſt part of the *French* nation: Both opinions agree in moſt particulars, as that the ambergreaſe is waſh'd off from the rocks, that it is digeſted and concocted by the ſun, *&c.* and that they only differ in that the one ſpecifies bees, the other birds; the one bees-wax, the other the excrements of birds: yet both ſuppoſe them winged animals: The ſum of the latter opinion is, that there is a certain kind of bees in thoſe places, where ambergreaſe is uſually found; that theſe bees make their hives upon the rocks and about the ſea-ſhore, (but others propoſe the matter more ſubtily, by ſaying, that the bees do not make their hives upon, but below the rocks, namely, in their caverns from whence the waves of the ſea waſh them off in the ſame manner as they did the excrements of the birds; and they are afterwards digeſted,

geſted, attenuated, toaſted and alter'd in ſuch a manner by the continual agitation of the waves, the acceſſion of the ſalt water, and the co-operation of the ſun, as at length to become ambergreaſe, and under this form thrown out upon the ſhore: The ſticklers for this opinion endeavour to eſtabliſh it farther from that white, tenacious and viſcous ſubſtance, that is uſually precipitated from the eſſence of ambergreaſe, and which they take to be ſomething waxy.

That both theſe opinions are falſe is plain from the following circumſtances. 1. A ſufficient quantity of ambergreaſe is found in theſe places, where there are neither birds nor bees. 2. And even where there are few or no rocks. And 3. where theſe rocks have neither excrements of birds, nor bees-wax.

In particular; that the opinion on the excrements of birds is falſe and erroneous, does moreover appear from the following particulars. 1. The parts of birds, ſometimes found in ambergreaſe, are not the bills or claws of geeſe, but only ſmall and fine particles. 2. It does not appear how the bills, claws, *&c.* of other birds ſhould paſs into the body, or excrement of theſe. 3. The eating of fragrant herbs does by no means produce a fragrant dung. 4. It is contrary to univerſal experience, that the ſun ſhould by digeſtion give a fragrant ſmell to ſuch excrements; on the contrary, it rather cauſes and promotes putrefaction; by which means there is produced not the fragrant ſmell of ambergreaſe, but a nauſeous ungrateful ſtench; eſpecially, if there is a great collection of ſoft excrements, as there is often large and viſcid maſſes of amber; or even the ſun extracts the fragrant particles, when ſmaller maſſes of dry excrement, ſuch as that of ſeveral birds, are expoſed to its beams; ſo as at length to appear quite dry, like a lean earth. 5. This dung of birds in its greateſt part, if not diſſolved by the waves, would at leaſt be divided into minuter parts, tho' never ſo ſtrongly irradiated by the ſun and moon. 6. We are not to ſuppoſe that the waves would wait, till the excrements were ſufficiently concocted and digeſted by the ſun and moon, but waſh away indiſcriminately both the freſh and digeſted excrements; and the former would the more readily be diſſolved and diſſipated in the water; but then whence could we have theſe large, uniform ſolid pieces of amber, as ſometimes to weigh a quarter of a hundred, half a hundred, nay, a whole hundred weight. 7. Such as fiſh for amber, know of no ſuch birds and their excrements. 8. Should we conclude from the bones, bills and claws of birds, found in ambergreaſe, that it is their dung, we might equally infer, that

that it is the excrement of ſhell fiſh and other fiſhes; ſince many parts of theſe are found therein. 9. The ſubſtance and natural compoſition of ambergreaſe, nay, in fine, its chymical analyſis contradict this opinion; ſince it yields no ſigns of excrements.

And this may alſo ſerve to confute the other opinion about bees-wax: for 1. No ſuch kind of bees and bee-hives have been mentioned by any one, either in *Aſia* or *America*, excepting *Denis* and *Monconys*, and ſuch as take their relations on truſt. 2. Suppoſing never ſo many bee-hives in the rocks near the ſea, every one knows that honey is apt to diſſolve or gradually run in water, even tho' it ſtood immoveable and ſoaking therein; but much more ſo ſhould it be ſtirred and agitated, as muſt be the caſe in a ſtormy ſea. 3. Conſequently this honey could by no means become more ſolid, firm, and compact, much leſs be collected into that large uniform maſs like ambergreaſe. 4. The honey thus diſſolved and waſhed away, we ſhould have nothing left but empty honey-combs. But where do we ever find parts of ſuch figure in ambergreaſe.

Borelli, it is true, *Obſervat. Med. Phyſ. Cent.* IV. *Obſervat.* 66. rejoins, that theſe honey-combs are afterwards fill'd with ſeveral things from the ſea, and ſo become ſmooth and firm: But this is a giving up the origin he contends for, *viz.* That ambergreaſe is derived from bees only, ſince the honey-combs could not be filled with any wax from the ſea, nor fortuitouſly with ambergreaſe; but if we are to ſuppoſe them filled with any matters, which yet remains to be proved, they muſt be of different natures, according as this or the other thing happens to lodge therein: And muſt all theſe different ſubſtances be inſenſibly converted into true ambergreaſe? Strange notions indeed. 5. But ſhould any one object, that the wax is collected into one maſs by the heat of the ſun, the anſwer is: This does not appear from experience, be the honey-combs never ſo much expoſed to the ſun. And ſuppoſing the wax thus collected into a maſs, yet it would have a uniform appearance, and remain of that appearance, which does not agree with ambergreaſe in its variety, appearance, and relation. 6. Wax never of itſelf takes fire, before it is melted; but this ambergreaſe does. 7. Wax and honey have and retain their ſpecific ſmell, which does not at all agree with that of ambergreaſe. 8. Chemiſtry alone evinces, that in examining ambergreaſe, it gives no ſigns of honey or any waxy ſubſtance; nor can M. *Lemery* himſelf, *in loc. citat.* & *in ejus curſu Chymiæ*,

prove that white and viſcous matter which precipitates in the eſſence of ambergreaſe, and on which he principally builds, provided he examine it more accurately, to be a waxy matter. Dr. *Kæmpfer* in his *Amœnitat. exotic.* p. 632, 633, 634, who himſelf was in *India*, plainly contradicts theſe authors; eſpecially if they add this groſs circumſtance, namely, that there has been ambergreaſe found with crude honey in it: For, ſays he, 'All the modern *French* authors are miſtaken, who follow 'Denis in this matter.

To come now down to the vegetable kingdom: *Sylvaticus in Pandect.* affirms that ambergreaſe is a gum; but this opinion is ſhortly confuted by only conſidering its compoſition and conſtituent parts; it neither ſhews itſelf a gum-roſin, ſome part of which at leaſt ſuffers a ſolution in water; nor much leſs as pure gum, which ſhould diſſolve entirely in water: But the contrary rather plainly appears in ambergreaſe; conſequently it is no ways gummy.

Others hold ambergreaſe for a roſin, *Alexand. Geraldini in Itinerar. ſuo ad Pontif. Leo. X. ex Libavio lib.* IV. *Singular. cap.* 2. *in Scholiis p.* 320 *a Dno. Boyle*, Vide *Phil. Tranſ. No.* 97. *p.* 6113 *and ſeq.* or for a balſamo-reſinous matter of certain unknown trees near the ſhore, from which it trickles and runs down into the ſea; where floating about, it is ſaturated with ſalt-water, and at the ſame time digeſted by the ſun, and at length concocted into true ambergreaſe. But this opinion ſavours much of the fable: Becauſe, 1. Such trees ſhould be planted very near the ſea, if the roſin muſt trickle down into it: 2. They muſt ſtrike their roots very deep, leſt from their nearneſs to the ſea, they ſhould be undermin'd or quite overturn'd and waſh'd away by the continual beating of the waves upon them. 3. If each trickling drop or little maſs ſhould fall into the ſea, and be immediately ſurrounded by the water, their union into large lumps, ſuch as ambergreaſe is found in, would become very difficult, if not impoſſible; and it is triffling to call in the aſſiſtance of the ſalt-water, the digeſtion and concoction of the ſun in that troubled and cold element, the ſea. 4. Sailors, and ſuch as fiſh for ambergreaſe, whether natives or ſtrangers, in the places where it is found, know nothing of theſe trees; as the celebrated *Georg. Eberhard Rumpffius* has alſo abundantly ſhewn. *Valentini Oſt-Indiſche Sendſchreiben ſub* N° XI. p. 56. 5. In fine, its not mixing with expreſs'd oils, and ſeveral other things manifeſtly ſhews that it is no vegetable roſin.

Averr-

Averhoes, in *Colliget. cap.* 56. affirms, that ambergreaſe is a ſpecies of camphire; but as he did not underſtand what camphire was; and as they greatly differ in volatility, ſolubility, colour, ſmell, and in ſeveral other properties, they muſt be as oppoſite as light and darkneſs.

Others on the contrary hold ambergreaſe for the produce of a certain fruit, (*Nic. Monard. de ſimplicibus Medicam. Edit. Plantin. Antv. p.* 13.) which the whales are fond of and ſwallow down, and from which there is generated in their bodies a perfect ambergreaſe; but this chimera deſerves no anſwer.

Julius Cæſar Scaliger and *Serapius* (*Juſti Fidi Klobii ambræ hiſt. p.* 18.) took ambergreaſe for a ſpecies of muſhroom growing at the bottom of the ſea, from which at length looſen'd, it floated on the ſurface, and was thrown out on the ſhore; but this opinion refutes itſelf; becauſe, there is no muſhroom without ſome diſtinguiſhing characteriſtic form; a circumſtance that ill agrees with ambergreaſe.

Libavius lib. 4. *ſingularium cap* 1. and *Weckerus in ſpeciali lib. ſect* 2. *p.* 79. It. *Sylvaticus in Pandect.* take ambergreaſe for the froth of the ſea: But the following circumſtance alone is ſufficient confutation; namely, that in thoſe places, where the ſea foams moſt violently, or where its froth is in greateſt plenty, there no ambergreaſe at all is found; which, were their hypotheſis true, ſhould be moſt plentiful there.

Hieronymus Cardanus, *de ſubtilitat. p.* 284. takes amber for *ſperma ceti*: But how widely they differ is ſufficiently evident to every one.

Eichſtadius, in *lib. de confect. alkermes cap.* 12. and *Fragoſus in lib. de medicam. ex Indiâ in Europ. delatis cap. de ambaro p.* 89. affirm that ambergreaſe is the liver of ſome fiſh: But every body knows, that the liver has its *capſula* or *parenchyma*, and conſiſts of filaments of veins and arteries, which are by no means found in ambergreaſe; not to mention that what is got by diſtillation from a liver is quite different from what ambergreaſe yields.

Moſt who are ſticklers for the marine kingdom, as it is called, were of opinion, that ambergreaſe is produced from fiſhes; yet theſe again run into various opinions, reducible to three general claſſes, which afterwards ſubdivide into ſeveral ſmaller branches.

To give ſome idea of this matter. 1. Some affirm that ambergreaſe is manifeſtly generated in fiſhes. 2. Others with more reaſon, that fiſhes ſwallow it floating on the ſea.

In fine, thoſe of the third claſs determine nothing on either ſide, and only affirm, that ambergreaſe is found in fiſhes; but whether generated in, or only ſwallowed by them, they mention not. From theſe three principal opinions have afterwards aroſe ſeveral ſubdiviſions, as to the fiſhes, the ſwallowing and the production of ambergreaſe; ſome affirming, that it is found in the larger fiſhes only; but others, in the ſmaller too; ſome, that it is found in all ſorts of whales; others on the contrary, in a certain ſpecies of them; tho' theſe again, differ as to the denomination of this ſpecies. They likewiſe differ as to the ſwallowing of it: For, ſome affirm that it is greedily ſwallowed by all ſorts of fiſhes; but others, by one ſpecies only; ſome, that fiſhes die of it; but others, that it does them no hurt; ſome, that it is ſoon thrown up again, after being ſwallowed; others that it is thrown out with the *fæces*: And others further contend that it is likewiſe devoured by other animals. And thus in like manner there are no leſs different opinions as to its production; both as to the manner and place where; and whether it be an excrement, or recrement, *&c.*

The Dr. only touches upon the chief of theſe different opinions, thinking it both needleſs and of little uſe to conſider each in order.

The celebrated *Rumpffius in loc. citat. Valentini ſub* N° 8. p. 50. in his letter from the iſland of *Amboina* to M. *Ten Rhyne*, and which is verified by *Gabriel Nakke* and others, ſays expreſsly, that not only the larger whales, but likewiſe ſmaller fiſhes and even birds and boars, and ſome likewiſe mention foxes, do very greedily ſwallow proportionable pieces of ambergreaſe, where they can come at it, which they afterwards throw up again: and hence he ſays it is, that there have been ſuch various opinions about ambergreaſe; ſome aſcribing its production to whales, and others to hogs; tho' it is only fortuitouſly found in them, as having been ſwallowed by them.

Of ſuch as hold, that ambergreaſe is found only in one ſort of fiſh, and in a certain ſpecies of whales; ſome who call this fiſh azel (*Geſner de aquatil. & quidem de cetis diverſis lib.* 4. *p.* 204.) affirm, that it very greedily ſwallows it, but ſoon after dying of it, it is carefully ſought for by the fiſhermen. Others call this fiſh *Mokos* (*Kæmpferi Amænitat. exotic. faſcicul.* 3. *p.* 635.) and relate that it is upwards of three or four fathoms in length; that it lives in the *Eaſt-Indies*, and is taken

taken about *Japan*. Others, as M. *Andr. Cleyer*, who liv'd a long time in the *Eaſt Indies*, call this fiſh *cetum ambrophagum*, or the ambrivorous whale; and the ſaid M. *Cleyer* ſent M. *Mentzelius* a draught of it; which, together with M. *Cleyer*'s account, was publiſh'd *Ann.* VIII. *Dec.* 2. of the obſervations of the Academy *Naturæ Curioſorum*. Others affirm, that it is from a whale of the *Lamia* kind, *Valentini Oſt-indiſche Sendſchreiben p.* 50. and others again, as Mr. *Dudley*, imagine, that ambergreaſe is ſolely produced from the *ſperma ceti* whale.

Many who eſpouſe neither ſide, affirm, that ambergreaſe is generally found in all ſorts of fiſhes, and not in any one particular ſort; nor do they decide the controverſy, whether it be ſwallowed by, or generated in them; only in this they differ, that they do not aſſign for it one and the ſame place in the fiſh: For, ſome, and that the greateſt number affirm, that it is contained in the whale's ſtomach; others, in the inteſtines; whence it is that ſome of theſe affirm, that the whales throw up the amber; but others, that they diſcharge it with the *fœces*: All theſe opinions, however, come to the ſame thing, and tacitly agree; *viz.* that ambergreaſe is not generated in, but rather ſwallowed by fiſhes: Since it is ſelf-evident, that whether a thing be diſcharged by the mouth or *anus*, it muſt needs previouſly have been in the ſtomach; and what is diſcharged at the mouth, does certainly come from the ſtomach: If, therefore, the ambergreaſe in the ſtomach be not thrown up, but retain'd, then it may naturally paſs farther into the inteſtines, and at length be quite diſcharged: And it cauſes no real difference whether it be found in the ſtomach or inteſtines; or, whether it be diſcharged upwards or downwards: It is a thing well known, that naturally there is nothing in the ſtomach but liquid juices; on the contrary, every thing elſe, eſpecially the ſolid and compact ſubſtances muſt neceſſarily enter by the mouth, and ſo be ſwallowed down: If ambergreaſe therefore be really found in the ſtomach or inteſtines of a whale, it is unreaſonable to imagine, that it was naturally produced there: Is it not rather more agreeable to nature, that this ſubſtance be by no means generated in this animal, but originally exiſting without it, and afterwards ſwallowed as food? Since many credible authors, tho' they differ, one in affirming that it is thrown up by whales; another, that it is diſcharg'd by the *anus*; a third, that it is found in the ſtomach: and a fourth

fourth, in the inteſtines; yet they agree in this circumſtance, as a certain and undoubted truth, that ambergreaſe is ſwallow'd by fiſhes, and by no means generated in them.

The aforeſaid circumſtances however, as that ambergris has been diſcharged by whales both upwards and downwards, and that ſome of it has been found in the ſtomach and inteſtines, have betrayed ſeveral into the miſtake, eſpecially, the inhabitants of *Madagaſcar*, as alſo many fiſhermen and ſailors, that ambergreaſe is nothing other than the excrement of the creature. The *Japoneſe* in their own language call that found in fiſhes, or diſcharged by them, *Kuſura no fuu* (*Kæmpf. in loc. citat. p.* 635.) which is ſaid to denote the dung of whales.

To affirm that ambergris is the excrement or dung of whales, becauſe diſcharged by them, ſeems not concluſive; as is manifeſt from the following inſtance: Running mercury, or pills of *regulus* of antimony, call'd the perpetual pills, are ſucceſſively taken and diſcharged by 10, 20 or more men; one diſcharges them immediately; a ſecond retains them a longer time, and a third probably dies with them; as the Dr. obſerved in giving mercury in the *Iliac* paſſion: Now if both the mercury and pills were either diſcharged or found in the body, a pure mercury and pure *regulus* of antimony, no one therefore would take them for any animal matter, or call them an animal excrement: The ſame thing alſo holds of the deglutition and ſubſequent ejection of ambergris. It is here ſufficient to obſerve, that ambergris is a foreign body, floating upon the ſea, which fiſhes do greedily ſwallow down, as a tidbit; in the manner as muſhrooms, truffles, *&c.* are ſwallowed by men, but diſcharged again indigeſted: That ambergris is ſomething foreign to fiſhes and indigeſtible, appears from ſeveral relations. The abovementioned fiſh, *azel*, is expreſsly ſaid to be fond of ambergris, and to die of it ſhortly after; but to inſiſt only on what is generally related; fiſhes diſcharge the ambergris, they have ſwallowed down, either at the mouth or *anus*; and it is an undoubted truth, that it is very rare to find any ambergris in the ſtomach or inteſtines of fiſhes. *Monardes* ſomewhere relates, that in his time in the *Canary Iſlands*, 100 *lb.* weight of ambergris was found in the inteſtines of a whale; and that from that time a great number of whales, ſmall and great, were killed, but no ambergris found in them. To this the Dr. adds, that in ſeveral hundreds of whales there is no ambergris, and that what is ſometimes found in them, was either ſwallowed a little before, or caſually retained there thro' its compacter bulk

or

or ſome other impediment, if not diſeaſe, whereby it was incapable of paſſing the inteſtines and being diſcharged.

It therefore remains an undoubted truth that ambergris is ſwallowed by ſeveral ſorts of fiſhes, but in particular, by whales; that it is afterwards diſcharged either by the mouth or *anus*; or ſometimes alſo retained in their bodies, and after death found either in the ſtomach or inteſtines; as all theſe circumſtances have for a long time back been unanimouſly verified by ſeveral perſons of undoubted credit: But we are not thence to conclude, that ambergris is produced in the ſtomach or inteſtines of theſe fiſhes; or becauſe it has for ſome time been retained in their bodies, that therefore it is ſome animal matter, referable to the animal kingdom.

The laſt opinion, reducible to this head, makes ambergris an animal recrement, and that of the whale kind; or, a matter, produced in the whale, as ſomething peculiar to it; in the ſame manner as caſtor, muſk, civet, bezoar, *&c.* in other animals.

A Continuation of the Account of Ambergris; *by the ſame.* Phil. Tranſ. N° 434. p. 371. *Tranſlated from the* Latin.

IN *Phil. Tranſ.* N° 385. and 387. are publiſhed two particular accounts of ambergris, tranſmitted to the *Royal Society* from *America*; the one by Mr. *Boylſton*, and the other by Mr. *Dudley*: Dr. *Neuman* here only gives a ſummary of both theſe accounts.

1. Ambergris, according to Mr. *Dudley* is found only in the *ſperma ceti* whale.

2. It conſiſts of balls or globular bodies, of various ſizes, from about 3 to 12 inches diameter, each ball weighing from 1 and $\frac{1}{2}$ *lb.* to 22 *lb.*

3. Theſe balls lie looſe, in a large oval bag or bladder, 3 or 4 foot long, and 2 or 3 feet deep and wide, and almoſt in the form of an ox's bladder, only the extremeties more acute, and reſembling a black-ſmith's long pair of bellows.

With 4. A ſpout running into, and thro' the length of the *penis*. Beſides

5. A *duct* or canal, opening into the other end of the bag; and

6. Proceeding from the region of the kidneys.

7. This bag lies juſt over the *teſtes*, which are above a foot long, and is placed lengthwiſe at the root of the *penis*, about 4 or 5 foot below the navel, and 3 or 4 foot above the *anus*.

8. This

8. This bag is almoſt full of a deep orange-colour'd liquor not quite ſo thick as oil, and ſmelling ſtrong, or rather ſtronger; of the ſame ſcent with the balls of ambergris, which float and ſwim looſe therein.

9. The inſide of the bag is very deeply tinged of the ſame colour as the liquor, which may alſo be found in the canal of the *penis*.

10. The balls ſeem to be pretty hard, while the whale is alive, ſince there are frequently found, upon opening the bag,

11. Large concave ſhells of the ſame ſubſtance and conſiſtence with the balls themſelves, that have ſcaled off from them.

12. And the balls themſelves ſeem to conſiſt entirely of ſuch diſtinct coats, incloſing one another, like the coats of an onion.

13. The number of balls never exceeds 4 in a bag, according to Mr. *Atkins*; who affirms that in a bag, where he found one, that weighed 21 pounds, the largeſt he ever ſaw, there was no other.

14. The whale-men obſerve, that only the old, well grown and male whales, contain ambergris: Yet at the ſame time Mr. *Atkins* relates,

15. That he never ſaw, nor certainly heard of a *ſperma ceti* female, taken in his life, the cows of that ſpecies being much more timorous than the males, and almoſt impoſſible to come at, unleſs when haply found a-ſleep, or detain'd by their calves. This is certain, the boats can never come near them, when they are awake, being very ſhy and fearful: Beſides, Mr. *Atkins* affirms,

16. That to one *ſperma ceti* whale that has any of theſe balls, there are two, that have nothing of the abovemention'd deep orange-colour'd liquor in their bags; and in fine,

17. Mr. *Dudley* in the very beginning of his account, boaſts that after ſo long an incertainty about ambergris, he at length diſcovered this ſecret of nature to be no other than an animal production, generated in the body of the *ſperma ceti* whale, in the ſame manner as muſk, *&c.* in other animals. And about the end of his account he ſays, I hope the *Royal Society* will aſcribe to me the honour of the diſcovery, or at leaſt that of the aſcertaining the origin and nature of ambergris: Yet,

18. A little before he owns, that he dares not pretend to give his own opinion upon the point, but contents himſelf with relating matter of fact.

19. The

19. The greateſt part of which account he had from Mr. *Atkins*, who uſed the whale-fiſhery for 10 or 12 years together, and from ſome other whalemen.

The ſum of Mr. *Boylſton*'s account, conſiſts of the following particulars.

That ambergris is found in no other than the *ſperma ceti* male whale; and in one about 20 pounds, more or leſs, of that drug. That it is ſcarcely found in one of a hundred of them. That it is contained in a *cyſtis* or bag. That they have ſometimes found this bag empty, and yet entire. That this bag is no where to be found but near the genitals of the fiſh. That the ambergris, when firſt taken out, is moiſt, and of an exceeding ſtrong and offenſive ſmell. But whether ambergris be naturally or accidentally produced in this fiſh; he leaves it to the learned to determine, *&c.* in fine that he had his account, only from the fiſhermen or whale-men.

From all which, it appears; 1. That the 2 accounts above-mention'd, agree in ſome reſpects; and 2. Differ in others; yet conſiſtently with the principal queſtion.

Both accounts agree, 1. That ambergris is found in the *ſperma ceti* male whales only. 2. That it is contained in a *cyſtis* or bag. 3. That there are lumps of it of 20, 21 or 22 pound weight. 4. That this *cyſtis* lies near the genital parts of the fiſh. 5. That ambergris newly taken out, is moiſt, and of an exceeding ſtrong, and offenſive ſmell; in fine, 6. That they owe their accounts and new diſcoveries to the fiſhermen and the whalemen.

They differ in the following circumſtances. 1. Mr. *Dudley* affirms that this *cyſtis* has a duct or canal from either end of the bag; one in the upper part of the bag, coming out of the kidneys, another in the lower part, running into and thro' the length of the *penis*: On the contrary, Mr. *Boylſton* affirms that this *cyſtis* has neither inlet nor outlet. 2. Mr. *Dudley* affirms, that tho' in ſome fiſh no ambergris be found; yet they always have orange-coloured liquor, mention'd above: But Mr. *Boylſton* affirms that this bag is ſometimes found quite empty. 3. Mr. *Dudley* ſays, that for one fiſh that has ambergris, there are 2 in which they hardly find any: But Mr. *Boylſton*, that ſcarcely one in 100 has any ambergris.

Theſe 3 differences, however, may be eaſily reconcil'd: As to the firſt, that according to Mr. *Boylſton*, the bag hath no inlet or outlet; this will only ariſe from the negligence of the fiſherman, who gave the account, and who did not obſerve every

particular ſo accurately as Mr. *Atkins* whale-man did: For how could the bag be found ſometimes full, and ſometimes empty, if it had neither inlet nor outlet? As to the 2d difference, namely that the bag was ſometimes found empty, this may be underſtood either of the balls only, or of the liquor likewiſe: But it is not directly contrary to Mr. *Dudley*'s account, ſince both may be natural, or happen ſo at that time: In fine, the 3d difference has no manner of difficulty, but both accounts agree, in that the pretended ambergris is found in ſome only, but not in all male whales; and as Dr. *Neuman* takes it, a certain is put for an uncertain number.

That the concrete, taken for ambergris, is true ambergris, and not ſome other very different natural ſubſtance, Dr. *Neuman* denies, and entirely agrees with Mr. *Prince*, who, according to Mr. *Dudley*, takes the aforeſaid *cyſtis* to be the urinary bladder, and the ambergris-ball, to be a certain concretion, formed out of the greaſy odoriferous ſubſtance of the aforeſaid liquor, contain'd within it. For his particular Dr. *Neuman* declares, 1. That the *cyſtis* or bag is no other than the urinary bladder of the whale. 2. That the pretended ambergris, found therein, is only the ſtone in the bladder; and 3. That the ſtrong ſcented liquor contain'd in the bag, is the whale's urine. That this bag is the urinary bladder, appears from its deſcription; as that it is 3 or 4 foot long, 2 or 3 foot broad, and almoſt in the form of an ox's bladder, or like a blackſmith's long bellows; eſpecially when we add its ſituation under the navel, over the teſticles, at the root of the *penis*; and its connection with the *penis* and kidneys, and how it is furniſhed with a ſtrong-ſcented liquor, without any of the pretended ambergris: And ſuppoſing, according to Mr. *Boylſton*, that the bladder is ſometimes obſerv'd to be empty (tho' he expreſſes himſelf leſs accurately on this head, it being probable, he meant, empty of ſtones) it would not be altogether impoſſible or unnatural, ſince the whale might happen to be killed ſoon after it had made water: What farther confirms it to be urine is, that it is found as all other urine is, in the *urethra* of the whale. That the balls have the ſame exceeding ſtrong and offenſive ſmell with the liquor, and conſiſt of diſtinct ſeparable coats like an onion, are circumſtances, common and natural to all animal ſtones as well of the urine as gall-bladder: Such a *calculus* then, is no longer to be reckon'd ambergris; nor the bladder, a peculiar *cyſtis*; nor the urine a peculiar liquor: This is farther verified from what is ſaid by Mr. *Dudley*, that the whale-men obſerve the full grown

grown and large whales only to contain ambergris, or rather a *calculus* according to Dr. *Neuman*; and by Mr. *Boylſton*, that it is ſcarcely found in one of 100: And theſe 2 circumſtances are both natural and common to men and beaſts; *viz.* that the old are ſooner troubled with the ſtone than the young; and that as ſcarce one man in a hundred is afflicted with the ſtone, the ſame may alſo happen in brutes: Beſides, the number of other animal ſtones is precarious, being ſometimes more and ſometimes fewer; and they may conveniently become round by the continual agitation in the urine, tho' the Dr. has obſerved, that the figure of *calculi*, as alſo of bezoar-ſtones, commonly ariſes from that of the *nucleus*, round which the firſt cruſt is laid; at other times their figure is owing to a greater or leſs ſpace, and a greater or leſs degree of motion: For, if they be more in number, as to poſſeſs the whole capacity of the bladder, or incapable to float and move freely about therein, they then are ſeldom round, but commonly unequal and informed, and thus they may eaſily ſeparate into flakes or coats, if they are many in number, or happen to ſtrike againſt each other: Nor does the circumſtance, that the male whales only contain theſe balls, weigh any thing with the Dr. ſince it is poſſible, that the males only are troubled with the ſtone; or we may not have ſufficient experience of this matter as to the females; and that the more as it was ſaid, that the females or cows are hard to come at; and that hardly one in a hundred of the males, according to Mr. *Boylſton*'s obſervation, is troubled with the ſtone.

On the contrary, that very circumſtance mention'd by Mr. *Dudley* and Mr. *Boylſton*; *viz.* that ambergris, which the Dr. calls a *calculus*, is not found in all male whales, and ſcarcely in one of a hundred, and only in the old ones, ſhews that it is not ſuch a recrement, as muſk, *caſtor*, *&c.* and at the ſame time confirms the Dr's. opinion, that it is no other than a *calculus*.

For, 1. were their pretended ambergris a natural recrement, generated in theſe male whales, and were that bag, ſome peculiar *cyſtis* without the urinary bladder, and the concrete found in it ſome ſuch natural ſubſtance, as muſk in the animal, that yields that drug, civet in the civet-cat, and *caſtor* in the beaver, to all which Mr. *Dudley* likens this *calculus*, generated from a morbid ſtate of the animal; then all the male *ſperma ceti* whales ſhould have this concrete, as beavers have *caſtor*, muſk-goats, muſk, *&c.* and in that caſe the bladder would not be its natural place; and all whales of that ſpecies without exception

(not one in a hundred, or the old ones only) would always and necessarily contain such balls.

2. Were this concrete generated in these fishes, and natural to them in a healthful state, as *castor*, musk, civet, *&c.* to other animals; not only the *sperma ceti* whales in *America*, but also those in the *Spanish*, *French* and *British* seas, especially in the northern ocean, would have it: And yet no such thing has been observed.

3. Mr. *Dudley* affirms, that the largest ball of ambergris he ever saw weigh'd between 21 and 22 *lb.* which, for the size of the bladder, is a pretty large *calculus*: But the Dr. queries; whence those lumps of ambergris could proceed, which were not entirely round, or of any determinate figure, yet 6 foot long; and weighing 182 *lb.* and upwards; and not coated like an onion, or friable, or of an orange colour, much less of an ungrateful smell, and least of all fetid; but entirely unformed firmly compacted, of a grey colour, and sweet scented? And again, whence could pieces proceed, exceeding the former in weight and size, 10, 20 times and upwards, and not to be contained in the *cystis* of the biggest and oldest whale.

4. The Dr. farther queries, how this ambergris should ever come ashore; it could not proceed from live whales; because the only outlet of the *cystis* is thro' the *penis*, thro' which indeed small pieces might pass; but not such large lumps of ambergris, as are often found. But should it be alledged, that such pieces are got from dead whales, it would remain to account for their passing out of the bag; especially, since it is membranous and of a pretty firm texture; so that its bursting after death, and discharging these floating balls is no such reasonable supposition; and it is a chance but ambergris might be sometimes found floating about in its bag, but this is a thing that has never happened to any one.

5. The becks of birds, the claws, shells, and spines, of fishes and other adventitious matters, sometimes found in ambergris could not pass thro' the kidneys and *ureters* into the bladder, or pretended bag of ambergris.

6. And lastly, but waving all other arguments, Dr. *Neuman* adduces one, as an incontested principle, and his chemico-physical canon, by which he regulates himself in all natural disquisitions; and which never failed him, nor really can, if applied with judgment and discretion: And this was his principal motive of denying ambergris to be either an animal production or extract. It is a thing well known, that not only *castor*, mu-

a

and civet, but likewiſe all animals and their parts, of whatever denomination, ſhould exhibit themſelves ſuch in a chemical analyſis, eſpecially in an open fire or in diſtillation; and conſequently, neceſſarily yield an empyreumatic urinous ſpirit, or phlegm, or an animal fetid oil, or together with it an urinous volatile ſalt; and if all theſe not at once; yet ſome ſmall quantity, or at leaſt ſome ſigns of an urinous liquor or empyreumatic animal oil, which ſhall affect the ſmell and taſte, when theſe bodies are treated in the fire; as all animal bodies really do, and even their ſhells, theſe ſtony and earthy parts, do in this teſt betray their animal origin: And thus likewiſe ſhall the pretended balls of ambergris, which the Dr. ſuppoſes to be ſtones in the whale's bladder, plainly and ſufficiently ſhew by this teſt, that they are not only of a pure animal, but of a pure urinous original, as being generated from the whale's urine. But if any one on the contrary, ſhould take genuine ambergris, depurated from all viſible animal parts, and not ſwallow'd by any animal, and diſtil it in the ſame manner in an open fire, and examine what is got from it; he ſhall not find even the leaſt urinous part, nor empyreumatic animal oil, nor any thing animal, but quite different modified productions, a different phlegm, oil and ſalt, as ſhall be further ſhewn hereafter.

This, therefore, may be looked upon as an infallible ſign and an undoubted truth; that when in the chemical treatment of ambergris the leaſt quantity of an animal ſubſtance is got, whether oleaginous, or urino-ſaline; that this can by no means be produced from pure amber, but from ſome animal matter accidentally mix'd therewith, and which is always to be conſidered as ſomething adventitious, impure and not belonging to the compoſition of ambergris; whether the proceſs be made with ambergris that has, or has not been ſwallow'd before, but with which are mixed the becks of birds or other ſmall parts of animals.

But that ambergris ſwallow'd down by fiſhes, or other animals, ſuffers ſome degree of alteration, at leaſt ſome animal taint or foulneſs can hardly be denied; whether it be afterwards diſcharged by the mouth or *anus*, or whether it be found and taken out of dead fiſhes: And on that ſcore it generally ſmells ranker and appears of a darker colour; nay, experience has taught even to diſtinguiſh immediately by inſpection what has been ſwallowed down; on which account it has in ſome places been called the dung or excrement of whales, as was obſerved above.

And

And ſince the 2 abovementioned accounts, tranſmitted from *America*, are from whale-fiſhers and Merchants; nay partly from hearſay: Hence we are ſo much the leſs to regard them as unqueſtionably certain or new, on the true origin and nature of ambergris.

To come now to the laſt claſs, namely ſuch as hold ambergris for a mineral, or aſcribe its origin to the mineral kingdom; theſe may be divided into 5 opinions, each of which, in a certain ſenſe, may be approv'd of.

Hugo de Lindſchott, vide *Klobii ambræ hiſt.* p. 19. takes ambergris for an earth: But this is contrary to nature and experience and the properties of ambergris; unleſs earth be taken in a wider ſenſe, as one of the chemical principles, and not for a mere earthy matter; for, earth does not ſo eaſily take fire, nor ſo readily melt, nor admit a ſolution with ſpirits of urine, not to mention other properties of ambergris.

2. Others attending only to the properties of liquation and inflammability; as *Crato, in Conſiliis a Laur. Scholtzio collect. Column.* 1093, and ſeveral others, have taken ambergris for a native and true ſulphur; Dr. *Salmon*, affirms that it is a marine ſulphur: vide *London Diſpenſatory* p. 398. *Edit. Lond.* 1696. 8*vo*: And ſuch a denomination is paſſable, were no more meant than that ambergris partakes of an inflammable or ſulphureous principle; ſince not only the ancients, but many moderns give the denomination, ſulphur, to whatever takes fire; whether it be a mineral, vegetable, or animal ſubſtance; an oil, fat, roſin, wax, pitch, balſam, wood, coals, bitumen, ſpirit of wine, or whatever elſe it be: But ſince this general denomination expreſſes nothing ſpecific or determinate; and ſince at the ſame time it appears pretty evident, that a native and true ſulphur is meant, and not the inflammable principle; ambergris can be no true ſulphur. 1. Let ambergris be ſprinkled on live coals, and its ſmell examined whether it agree with that of ſulphur. 2. Let it be tried with a fixt alcaline ſalt, whether it yield a *hepar ſulphuris*, vitriolated tartar, *&c.* 3. Whether ambergris again reduce *regulus* of antimony into crude antimony. 4. Whether there is a *lac ſulphuris* from a ſolution of ambergris in quick-lime water, or an alcaline *lixivium*. 5. Whether from ambergris and mercury cinnabar be produced; and whether ſeveral other preparations, which are otherways made with true ſulphur, be obtainable with ambergris. And all the attempts and proofs to that purpoſe, will become vain and ineffectual, as *Libavius* has already ſhewn againſt *Crato*.

Some

Some at length come nearer the matter, and hold ambergris not only for a mineral, but for what it really is, a bitumen, or ſpecies thereof; but happen to differ in ſome circumſtances.

3. Some affirm that ambergreaſe is produced in aliquid form.

4. But others in a dry form.

5. Others again, that it is produced in the ſea in a viſcous form, or of a mean conſiſtence.

They likewiſe differ, in that ſome affirm, that ambergreaſe ariſes from under the ſhore and rolls down into the ſea: But others attempt to ſhew, that it ariſes from the abyſs, or rather from the bottom of the ſea.

1. Such as ſuppoſe ambergreaſe to have been originally fluid, or conveyed into the ſea in a liquid form, hold it originally in its nature for a liquid bitumen, or ſpecies of *Naptha*; and amongſt theſe are *Ebnſina*, *Simeon Sethi*, *Nævius*, *Avicenna*, *Agricola*, *Solenander*, *Bertinus*, *Libavius*, *Garcias ab Horto*, *Hadrianus Toll*, *Joh. Euſebius Nierenbergius*, *Franciſcus Hernandez*, and ſeveral others, who all hold, that ambergreaſe rolls into the ſea from a bituminous fountain, or fountain of *Naptha*; and they differ only in ſome little circumſtances, that are not material; for inſtance, according to *Avicenna* and *Pſellus*, it flows from lateral ſprings into the ſea: Others, on the contrary (among whom is *Nicolaus Chevalier*, who in 1700 deſcribed the large piece of ambergreaſe at *Amſterdam*) affirm that it ſpontaneouſly ſprings from the bottom of the ſea, and from *Kircher*'s reſervoirs of water is diſtill'd by the central heat, as it were, into the ſea.

2. Others, who hold that ambergreaſe is conveyed into the ſea in a dry form, affirm, that it is a kind of amber, or a real dry bitumen, which is conveyed in the ſame manner into the ſea, as the common yellow amber. *Andreas Cæſalpinus*, it is true, calls it a gem; however he claſſes it under the ſpecies of amber. The above quoted M. *Oelvin*, who took ambergris for a meteor, yet in a certain place of his treatiſe *Der Monathlichen curieuſen Natur-kunſt. Staats- und Sitten præſenten, zweytes ſtuck im Februar.* 1708. p. 56. affirms, that amber has a great affinity with ambergris; yet with this difference, that ambergris is found in warmer climates, where flowers and aromatics arrive to their higheſt perfection and moſt fragrant ſmell; whereas amber, on the contrary,

contrary, is only found in the cold northern climates, and in the colder *Baltic*; consequently, the latter is of a coarser and harder substance. In fine, the experienced and learned Dr. *Henckel*, vide *Bethesda Portuosa* p. 74. affirms, that ambergris, amber and asphaltum, or the oriental amber, common amber, and black amber, do not differ from one another in their essential principles, but only in their accidents and degrees; which last opinion agrees chiefly with experience.

3. In fine, such as suppose ambergris to run in a viscous form, of a mean consistence, into the sea, affirm, that it is at first like a soft pitch, or cow-turd in or upon the bottom of the sea, that gradually hardens: *Helbigius*, therefore, asserts, that ambergris is neither a gum, rosin, dung of birds, nor bees-wax, but a true viscous matter like pitch, that lies and grows, at the bottom of the sea, like sea-pitch; and this account was sent him by a merchant from *Batavia* who had seen it with his own eyes, *Ephem. Nat. Cur. Decur.* I. *Ann.* IX. *p.* 459. The above mentioned celebrated *Rumpffius*, in his letter to *Ten Rhyne*, *Valentini Oost-Indische send-schreiben*, *p.* 50. says, that this is certain, and that he advances nothing new, but his old opinion about the origin of ambergris; namely, that it is a kind of fat, that arises from the bottom of the sea, which at first is soft and viscid, but afterwards becomes hard by the saltness of the sea. *Aldrovandus* (*Mus. Metallic. lib.* III. *p.* 434.) after adducing several opinions, does at length conclude to the following purpose: 'But I reckon these distinctions of no weight; since I have 'asserted it to be a kind of bitumen, and not the dung of 'fishes or whales.'

Borelli, it is true, *in loc. citat. Observationum*, endeavours to form another objection, alledging that ambergris is no bitumen; that bitumen's have an ungrateful smell, and are not fit for internal uses: But in such cases we cannot always conclude from the genus to the species, nor from the species to the genus, as appears even from common amber; for amber is now universally acknowledged for a bitumen; and yet it has no ungrateful smell, but is us'd internally in a pretty large quantity and with safety; not to mention what is said in *Act. Erudit. Lipsien. An.* 1684. of a certain spring in *Poland*, that yields a liquid sweet-scented bitumen that is good both in physic and diet, and has a very fragrant and balsamic smell.

Nicolaus

Nicolaus Monardes, de ſimpl. medicam. p. 12. has already aid; 'there are various opinions about the origin of amber-greaſe; but the trueſt is, that it is a kind of bitumen, flowing from a ſpring, which as ſoon as it touches the air, immediately hardens in the ſame manner, as many other concretes, as coral, *&c.* which at the bottom of the ſea are ſoft.'

Joh. Faber. Lyncæus in Expoſit. in Rech. p. 565. affirms this, therefore, is very certain, that ambergris is no other than a bitumen'.

In ſhort, the greateſt part of the writers of natural hiſtory agree in the following reſpects, namely 1. That there are both liquid and conſiſtent bitumens. 2. That all bitumen's are referable to the mineral kingdom. 3. That a dry bitumen is no other than a tenacious fat of the earth, and which readily burns; and 4. That ambergreaſe is endued with the ſame properties; that conſequently it may without any contradiction, not only be a mineral, but likewiſe a peculiar ſpecies of bitumen.

Tho' ſomething more might be added, as to the original conſiſtence of ambergris; one affirming it carried into the ſea under a ſolid, another under a viſcid and tenacious, and a third under an entire liquid form; yet the Dr. ſees no abſolute neceſſity for it; ſince theſe three opinions, as being the chief of thoſe already adduced, may be eaſily united, and ſo combin'd together, as to be all true in a certain reſpect.

1. No one, perhaps, who has any ſolid knowledge of the origin of ſeveral ſubterraneous ſubſtances, will deny, that all bitumen's are originally liquid; even tho' they do not come into the ſea in a liquid form.

2. That both ambergris and amber muſt needs be originally, if not entirely liquid, yet for ſome time in a viſcous form; or even ambergris, when thrown out upon the ſhore, muſt at leaſt continue of ſuch a texture, that if it happen'd to harden in the ſea, it may eaſily be ſoftened by the ſun: For, otherways it could not include the becks of birds, ſhells of fiſhes and their parts; as many ſuch things are really found in ambergreaſe; and, as is pretty evident, ſuch a variety of inſects and other things are likewiſe found in amber.

3. In fine, it is pretty evident, that tho' ambergreaſe in an entire dry form appears like impure and ſomewhat ſoft amber, yet as a true bitumen.

What *Franciſcus Hernandez* ſays on this head is worth notice; namely that ambergreaſe runs from ſome ſprings of *Naptha* into the ſea, and by ſtormy weather is thrown out upon the ſhore; that it is a very fragrant inflammable matter, one time harder, another ſofter, ſometimes friable and ſometimes flexible; ſo as in ſome meaſure to be extenſile and flexible between the teeth and fingers like wax.

Dr. *Neuman* therefore thinks. 1. That ambergris runs out of the earth into the ſea like amber. 2. But not like *Naphtha* or petrol, but in a groſſer and flexile form, and probably often of a viſcid and clammy conſiſtence. 3. That under the firſt concretion or formation of ambergreaſe, it runs together a liquid bitumen or ſpecies of *Naphtha*. 4. That large pieces may be generated at once; but generally at firſt they are ſmall; but by the acceſſion of ſeveral new layers, the ambergris may be ſometimes round, ſometimes long, or even become of any unequal form: And during its formation it is always ſomewhat ſoft, whence many matters may be entangled in it; however from time to time it may gradually harden to the conſiſtence of wax: And ſince moſt ambergris appears thus in layers or coats; this circumſtance might miſlead thoſe in *America* to imagine that it is generated in the ſame manner as the bezoar or human ſtone; tho' they might reflect that various other ſubterraneous bodies, both the bituminous, as pit-coals, alum *&c.* and other minerals, as talc, ſlate, *lapis ſpecularis*, &c conſiſt of layers, as animal *calculi* do.

As to the reſt, the Dr. thinks it not ſo neceſſary to make ſuch deep and accurate reſearches into the original and primordial production of ambergreaſe: For, who can with certainty pretend to explain the manner in which common amber is produced? How metals, ſemi-metals, gems, fluors and many other mineral concretes are generated? Very few are the ſubterraneous mixts, of whoſe manner of production we have any certain knowledge, tho' we may form a conjecture about this and the other, nay, know the nature of very many of them: The manner of their generation ſeems to be no ſuch neceſſary and uſeful knowledge; it is ſufficient for us if we know what kingdom they are of, and if we know their internal compoſition and nature; and this latter manifeſts their kingdom: And phyſico-chemical experience is the moſt genuine and infallible touchſtone for all natural mixts.

To conclude, it is ſurpriſing that *Paulus Hermannus*, a man f ſufficient experience in the *Materia Medica*, ſhould make o mention of ambergris in his *Cynoſura*: This, however is a nore tolerable conduct than that of *Leonhardus Fuchſius*, who idiculouſly held all ambergris for factitious: But to wave this, et us farther conſider from whence it is brought to us.

Ambergris for the greateſt part comes from the *Eaſt-Indies*, rom *Madagaſcar* and thereabouts, the *Molucca* iſlands, *Sumara*, *Borneo*, *Cape Commorim* in *Malabar*, and the *Ethiopic* oaſts, which from *Sofala* to *Brana* is very productive of ambergris: It is alſo found in other countries, either as it may be lriven very far into the ſea, and conſequently thrown out again ıpon the ſhore in various places of the earth, or as it may be net with in the ſeveral ways abovementioned.

'Tis worthy taking notice, that this precious bitumen is often ound in very large pieces or lumps: But not to mention what *Faber Lyncæus* relates from *Gregory de Bolivar*, that ſometimes there are pieces of ambergris of 100,000 *lb.* weight; much leſs what *Garcias ab Horto* tells us, that there are whole ſlands full of ambergris; nor what *Iſaac Vigny* a *Frenchman* relates, that he knew a country, ſo rich in ambergris, that a houſand ſhips might be laden with it; all which ſeem to ſtretch the point too much, eſpecially the latter; Dr. *Neuman* ıdduces the following inſtances, which may ſerve to ſhew that here are really large pieces of ambergris.

In 1555 at Cape *Commorim* a piece was found of about 3000 *lb.* and which was then ſold for *Aſphaltum* or common bitumen: *John Hugo de Lindſchot* relates that there was found ıbout this Cape a piece of 30 quintals, which is equal to 150 hundred weight. *Monardes* and *Franciſcus Hernandez* make mention of pieces of 100 *lb.* weight; *Garcias ab Horto* ſpeaks of pieces as big as a man; beſides a piece, that was 90 hands in length and 18 in breadth: *Montanus* mentions a piece of 130 *lb.* weight, and kept by the king of *Satſuma* in 1659: In 1666 about Cape *Verd* at the river *Gambi* a piece of 80 *lb.* weight was thrown out, which was brought to *Holland*: In 1691 there was at *Amſterdam* a lump of 42 *lb.* *Daniel de Bruel* affirms that about *Malacca* a piece was found of 33 *lb.* at *Rome*, a piece as big as a man's head; and here and at *Loretto* and in other places in *Italy* are ſeveral valuable curioſities in ambergris, which abundantly ſhew that they were wrought out of very large pieces: The abovementioned *Vigny* had no ſmall piece, which he brought from *India*, and for which he

had 1300 *lb.* ſterling; *Kæmpfer* relates that in his time there was found in *Kinokuni*, a province of *Japan*, a piece upwards of 100 *Catti* or 130 *Dutch lb.* weight: The two brothers *Joh. Andreas* and *Marcus Matſperger* of *Augsburg* bought of *Robert Struzzi* at *Venice* in 1613 a piece that weighed 48 *lb.* and 8 *oz.* And to mention no more, we have a very recent and withal ſtrong, inſtance in that large piece of ambergris, which the *Eaſt-India* company in *Holland* bought in 1693 of the king of *Tidor* for 11000 dollars; which was at firſt of the form of a tortoiſe and weighed 182 *lb.* was 5 foot 8 inches thick, and 2 foot 2 inches long: The above named *Nicolas Chevalier* has deſcribed it at large in a ſmall treatiſe publiſhed at *Amſterdam* in 1700, with various copper plates of it in different views: From all theſe inſtances we can no longer doubt but that there are pretty large pieces of ambergris; ſo that it may be queried, how the accounts from *America*, that mention a bag, can agree with lumps of ambergris of ſuch bulk?

An Account of a new Engine *for raiſing* Water; *by* Walter Churchman. Phil. Tranſ. N° 434. p. 402.

1. IN this engine the horſes or other animals draw horizontally, in a ſtreight line and at right angles, whereby they exert their utmoſt force; by which means a far greater power is gain'd from the ſtrength of horſes, *&c.* than by their going round in a circle: For, by the twiſt and acuteneſs of the angles, they draw in towards the centre, whereby they waſte their power, and likewiſe ſhorten their levers: Beſides, their muſcles and tendons from their hinder legs all along their ſides to their necks are unequally ſtrain'd, as the duty is hardeſt on one ſide, even tho' their walk be large: Therefore, each of thoſe inconveniencies muſt be attended with pain to the animals when at work, and with a great loſs of their ſtrength.

2. A crank does not riſe quite $\frac{1}{3}$ of its circle; neither do the regulators or rods riſe or fall perpendicular, but obliquely; whereby an oval figure is formed by the piſtons motion in every cylinder, which occaſions great friction and a waſte of water, and every arm of it is continually varying in its power while working; as its lever is diſtant from the perpendicular line, and 2 of the arms (ſuppoſing it a quadruple one) as they croſs the perpendicular, are always drawing to and from their own centre, by which the power is not only loſt, but the time alſo. Farther, by the ſhortneſs of the ſtrokes, all the adjacent water is frequently mov'd contrarily, and by the often opening and ſhut-

PLATE. II.

Fig. II.

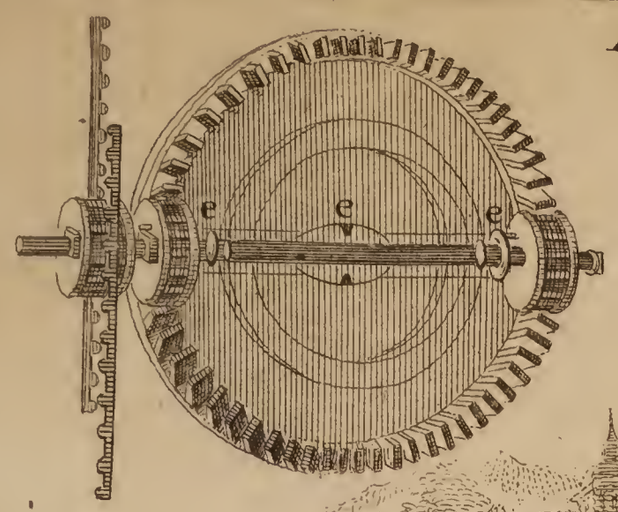


Fig. I.

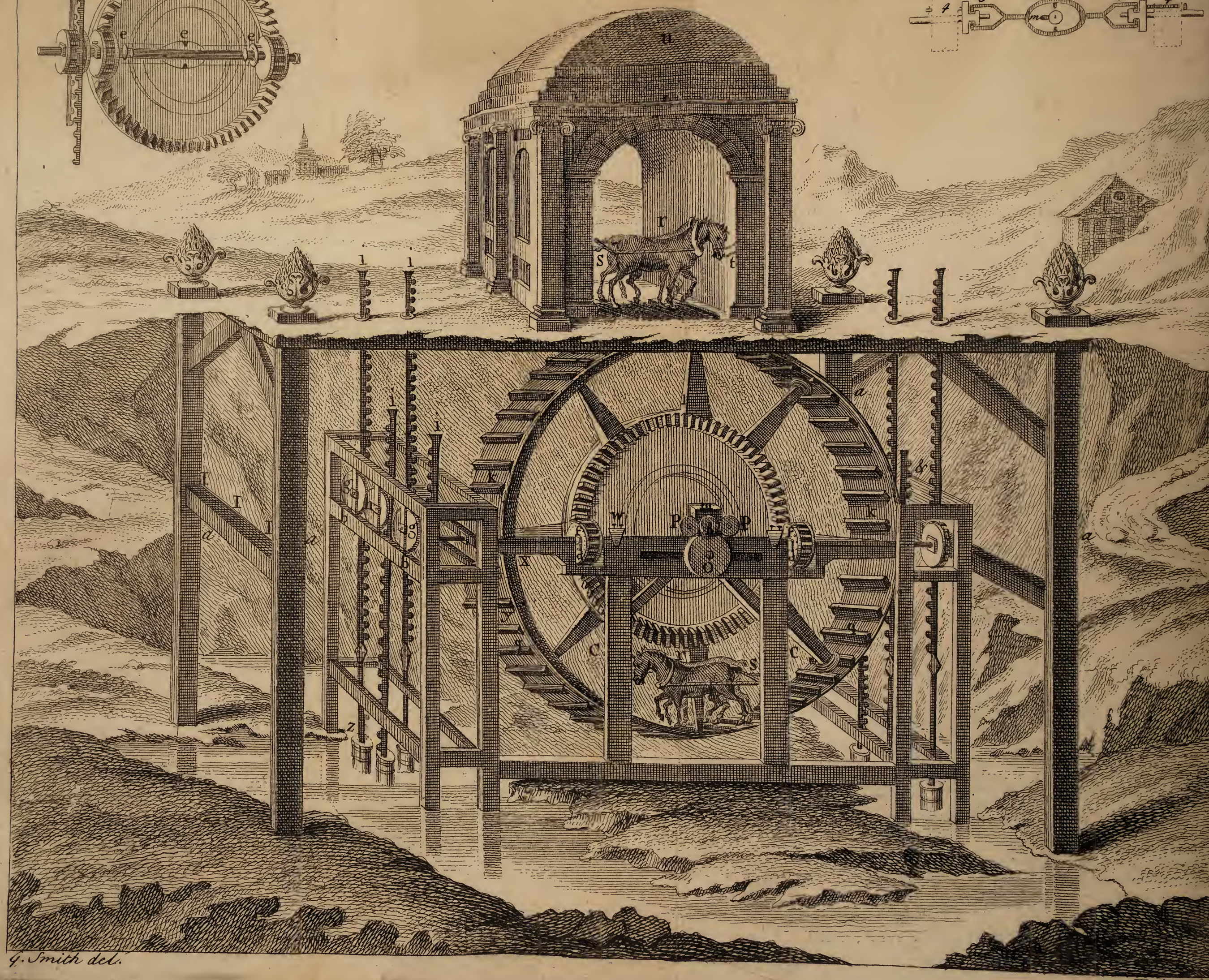


Fig III.

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Hulett Scu

ſhutting of the valves, there is alſo a great waſte of the water; beſides the many heavy bearings, frictions, ſurges and repairs belonging to it; all which inconveniencies and impediments being thoroughly conſider'd, there muſt certainly be requir'd a much greater power to work the ſame than by Mr. *Churchman*'s method, For, thereby a ſtroke of 24 feet will riſe; and by enlarging or diminiſhing the fixed wallower, you obtain a ſtroke of any required height, even to the extent of the preſſure of the atmoſphere: By this great advantage, the water riſes freer, and with greater velocity; and as the lifters or forcers riſe and fall exactly perpendicular, and with an equal continued ſtrain; and as the bearings alſo are fewer and lighter, conſequently, the friction in all theſe will be a great deal leſs than with the crank, *&c.* And laſtly, $\frac{7}{8}$ of that water which is always loſt by the ſlow opening and ſhutting of the valves will be ſav'd.

From the above conſiderations, and by the ſeveral experiments Mr. *Churchman* made on this occaſion, in order to know the real difference between theſe different ways of working, he finds that near twice the quantity of water will be rais'd to the ſame height, in the ſame time, with the ſame power, by his method, than with the beſt crank-work that has hitherto been erected.

aaaa (Fig. 1. Plate II.) repreſents the great frame, the ends of which under the pine-apples are to be contracted to the place of the little frame; ſo that the croſs piece at III may ſupport the 3 bearings, now ſhewn in the little one, for a better view only; *bb* the little frame on which the cap-braſſes are, which receive the turn'd gudgeons T in the 3 horizontal ſhafts; *cc* the ſtrong ſupporters by the looſe wallowers; *dd* the looſe wallowers, whoſe turned rounds geer truly with the cogs in the great wheel; *eee* the regulator, (Fig. 2.) which has a circular, direct and retrograde motion; *ff* the ſtrong ſhoulder or ſtud fixt to the ſhaft cloſe by the wallower, which ſtops this looſe wallower, when the end of the regulator comes againſt it, thereby confining it for 2 revolutions; after which it quits this ſtud, and does the ſame on the oppoſite ſide of the wheel; and ſo on alternately, to reverſe the motion of the ſtems in the different cylinders; *gg* the wheels, with their cogs, which alternately work the fixt wallower lying between them; *h* the fixt wallower ſuppos'd to be 4 foot in diameter (on a very ſhort ſhaft) whoſe rounds muſt be of caſt ſoft iron, and truly turned, to elevate and depreſs the racks to the height of 24 feet by its 2 revolutions; *iiii* the 4 lifters or forcers, behind each of which

which muſt be a ſmall leverage back wheel, truly fitted to direct the ſame to riſe and fall eaſily and exactly perpendicular, to avoid friction and loſs of water in the cylinders; *k k* the large vertical wheel, a ſmall ſegment of which comes thro' the floor in the dome for the 4 horſes to ſtand and draw on; *l m* (Fig. 3.) the arms and the main ſhaft of the ſame; the arms lie horizontal and the oval part is perpendicular; *n* the turned T gudgeon, with its collar and ſhoulder, both of which muſt claſp the rim of the under leverage wheel, to keep all firm and ſteady when in working; *o* the leverage wheel about 4 feet in diameter, with a braſs or iron rim ſuppos'd to be truly turned, and to have a ſtrong ſhort iron ſpindle thro' its center, and at each end a turned ſteel collar and ſhoulder, bearing on 2 caſt cap-braſſes exactly level, and ſunk into a ſtrong arch'd piece of timber well braced and ſupported for this purpoſe. And here it is to be noted, that in large engines and machines where the motion is regular, every heavy bearing ſhould have one of theſe wheels: For, they ſave power by greatly abating friction. Upon the principle of theſe leverage-wheels Captain *Rowe* has publiſh'd what he calls his friction-wheels, tho' ſubſequent to Mr. *Churchman*'s ſpecification thereof; *p p* two ſmall ſide leverage wheels, exactly fitted to the turned part of the great gudgeon, between the collar and ſhoulder; they are to be placed and key'd in ſuch manner, that their friction from the gudgeon may be alike when at work; *q q* the ſteps which the horſes feet preſs, about 8 or 9 inches broad, 2 inches thick behind, and declining to an edge, being deſigned to make level ground, and good footing for their hinder legs when they draw; *r r* four horſes only in view to avoid confuſion, all drawing horizontally in a ſtreight line, and at right angles, whereby theſe uſeful animals will ſoon be taught a new and pleaſant way of working to themſelves, a more advantageous one to their maſters, and of greater utility to the public; *s* the faſtening places behind the horſes, ſuppos'd to be ſtrong arms below in the ſupporter, and a croſs bar above, at both which you may place ſmall ſheeves or rollers, the upper part of them to be level with each horſes breaſt (when drawing); and the rope or ſtrap to come over the ſame, in order to keep a weight of 300 *lb.* (more or leſs) ſuſpended, one or two inches from a plank. By this method you will be exactly informed of the ſtrength of each horſe, how long it continues, and when to relieve him; as alſo when juſtly to correct the ſlothful one, whoſe weight reſting on the plank will always diſcover his lazineſs; *t* the faſtening places before. which are deſign'd to direct their

their heads; *u* the dome meerly for ornament; inſtead of which erect a workloft, over that a horizontal windmil: On the lower end of its upright ſhaft fix a ſpur wheel to work with the cogs of the great wheel, thereby to aſſiſt the horſes, or when there is a ſufficient force of wind to do their whole duty; *w* the coupling ſtaples with the braſſes; *x* the ſtrong catch which confines the great wheel to the frame; *y* the ſcrew or kay-band to confine all cloſe and light; *z* the cylinders which are ſcrew'd together at their ends out of ſight; and all the ſame ſort of work chiefly for uniformity in the draught.

An Abſtract of Meteorological Diaries; *with Remarks thereon; by Dr.* Derham. Phil. Tranſ. N° 434. p. 405.

THE following meteorological obſervations were made at *Naples* by Dr. *Cyrillus*, at *Bengal* by Mr. *Bellamy*, and at *Chriſtiana* in *Norway*, *by* —— communicated by M. *Kink. Ann.* 1727.

The following Tables of the barometrical and thermometrical obſervations are the more valuable, on account of their being made, as the Dr. ſuppoſes, with ſome of Mr. *Hauksbee*'s glaſſes.

The barometrical means at *Naples*, are both as they are ſet down by the illuſtrious obſervator himſelf, according to Dr. *Jurin*'s directions, and likewiſe as they are between the higheſt and loweſt ſtations of every month: Thoſe of *Norway* are in the latter way.

A Table *of the Barometrical Ranges at* Naples *and at* Chriſtiana *in* Norway, *in the Year* 1727.

	JANUAR.		FEB.		MAR.		
	Naples.		Naples.		Naples.		Norw
High	29.80		29.88		30.6		29.3
Mean	29.55	29.65	29.63	29.72	29.59	29.73	29.1½
Low	29.30		29.38		29.12		29 0

	APRIL.		MAY.		JUNE		
	Naples.		Naples.		Naples.		N rw
High	29.88		29.88		29.72		29 3
Mean	29.63	29.72	29.71	29.73	29.6	29 6	29.1½
Low	29.54		29.54		29.50		29

The Table *continued.*

	July.			August.			Sept.	
	Naples.		Norw.	Naples.		Norw.	Naples.	
High	29.80		29.7	29.80		29.7	29.88	
Mean	29.67	29.70	29.3½	29.55	29.63	29.5	29.59	29.72
Low	29.54		29.0	29.30		29.3	29.30	

	October.		Novem.		Decemb.	
	Naples.		Naples.		Naples.	
High	29.88		30.06		29.88	
Mean	29.50	29.66	29.59	29.75	29.59	29.65
Low	29.12		29.12		29.30	

A Table *of the Thermometrical Ranges at* Naples, Bengal *and* Chriſtiana *in the Year* 1727.

	Jan.	Feb.	March.		April.		May.	
	Nap	Nap.	Nap.	Beng.	Nap.	Beng.	Nap.	Beng
High	51.3	44.5	48.3	15.2	41.0	15.3	30.0	20.4
Mean	47.1	40.0	41.0	7.6	31.0	7.9	24.0	10.6
Low	43.0	35.0	34.5	0.1	21.0	0.6	18.0	0.7

	June.			July.		
	Nap.	Beng.	Chriſt.	Nap.	Beng.	Chriſt.
High	20.0	10.8	46	17.0	15.4	40
Mean	14.7	5.8	37	10.0	7.7	35
Low	9.5	0.8	29	3.0	0.1	30

	August.			Sep.		Oct.	Nov	Dec
	Nap.	Beng.	Chriſt.	Nap.	Beng.	Nap	Nap.	Nap.
High	21.0	15.4	45	25.0	10.4	43.5	47.0	50.5
Mean	11.1	7.8	35	19.7	7.7	32.2	43.7	43.2
Low	7.3	0.2	25	14.5	5.0	21.0	40.5	36.0

Theſe tables give an eaſy view of the barometer and thermometer in the ſeveral diſtant parts of the world ſpecified; which

which would have been very instructive, had they been observ'd throughout the year, as they were at *Naples*.

By the barometrical observations it appears, that the ascent and descent of the mercury is not so great at *Naples*, as in the more northerly climates: For, it was but twice in the whole year above 30 inches; and but thrice as low as 29.12 inches. And so in *Phil. Trans.* N° 321. Dr. *Derham* observed, that at *Zurich* the range is but about an inch; but at *Upminster* he finds the highest ascent to have been 30.44 inches and the lowest descent 27.44 inches, which is a range of 2 inches and ½: And by his account of the *Petersburg* observations in 1724. *Phil. Trans.* N° 424. p.107. it appears that the mercurial range there is 3.31 inches: And as to *Norway*, the observations are too few, and all made only in the summer months, whereby no good judgment could be form'd: And at *Bengal* they had no barometer.

By the thermometrical table we may judge of the heat and cold of the several places: For the right understanding of which, it is to be noted, that in Mr. *Hauksbee*'s thermometers, the point of extreme heat is 5 degrees above 0; the point of temperate 45 degrees below 0; and the point of freezing 65 degrees. But Dr. *Cyrillus* saith, it freezes with them at *Naples*, when the spirits are only got to 55 degrees. His words, which are to the following purpose, are remarkable: 'I take it to be 'worthy of your sagacity, and your study of nature, to account 'how it comes to pass, that with us water freezes when the spi-'rits in the thermometer are scarcely fallen to 55 degrees; 'whereas at *London*, not before they are at 65 degrees, where 'your point of frost is marked. And if our Philosophy can 'avail us any thing, it should seem that there is something or 'other requisite, with which our air is impregnated, and yours 'not, besides an intense degree of cold to congeal water, as in 'artificial freezing, not snow alone, but such as is mixed with 'salt, is to be employed.

And as it freezes at *Naples* at a warmer degree of the thermometer; so Dr. *Derham* observes, that at *Christiana*, the illustrious observer complains of the *vehement heat of the sun in July*, when the spirits were but at 36 and 34 degrees: In *August* at 25, 27, and 28 degrees, when he says *the weather was exceeding hot*. The Dr. thus distinctly mentions (as the author doth) *the heat of the sun*, and *the heat of the weather*; because they may not mean the same thing, he having been informed by the whale fishers, that in *Greenland* the heat of the sun is scarce tole-

tolerable on one ſide of the ſhip, when on the other it freezes hard.

At *Bengal* the heat at ſometimes ſeems to be very intenſe, by the thermometer being in ſome months more degrees above 0, than the point of extreme heat is: As particularly in *April*, *May*, and *June*, it was 6, 7, and 8 degrees above 0. But thoſe exceſſive heats are commonly in the afternoons, the forenoons being more temperate; and the temperature, or what they call cold there, is at the ſame time of the day: And the degree of the thermometer, at which they reckon it coldiſh, is about 15 degrees. And on *May* 2. at 8 o'clock in the morning Mr. *Bellamy* ſaith, (the thermometer being then at 20.4 degrees) *the morning was like winter weather in* Europe.

Whether this ſo different judgment of great cold at *Bengal*, when the thermometer was about 20 degrees, and of exceſſive heat at *Chriſtiana*, when it was but a little below that, *viz.* at 25 degrees, *&c.* Whether it ariſes from ſome prejudice, or from ſome extraordinary quality in the air, Dr. *Derham* leaves, as Dr. *Cyrillus* doth, to others to determine.

As to the weather, winds, rain, *&c.* of the ſeveral places Dr. *Derham* only gives a tranſient view of every month.

At *Naples*, *January* was a cool month, frequent rain with much thunder and ſtorms of wind. The rain amounted to 111 meaſures and $\frac{1}{2}$ (23 of which make an *Engliſh* inch in depth) which is 4 inches, 19 meaſures and $\frac{1}{2}$, or near five inches. *Veſuvius* was pretty quiet.

February was a drier month, the rain amounting only to 144 meaſures, which is but a little above $\frac{1}{2}$ an inch deep. The weather was for the moſt part cloudy, with ſome froſts. *Veſuvius* emitted a thick ſmoke.

At *Naples*, in *March* it was cold, with hail and ſnow on the mountains; the rain amounted to 101 meaſures, which makes 4 inches, 9 meaſures depth. The winds were in all the points. *Veſuvius* diſcharged rivulets of fire.

At *Bengal*, the 5 laſt days of *March* (which were all obſerved in this month) were fair, the wind S2.

In *April* the winds at *Naples* were much in the northerly points, cold, frequent thunder, the rain only 38 meaſures, which make 1 inch, 15 meaſures. There was no fire in *Veſuvius* the beginning of the month, but towards the latter end, diverse rivers of fire and ſmoke.

At *Bengal* the wind was much among the ſoutherly points, cloudy, ſome rain and thunder. The weather for the moſt part temperate, but great heats in the afternoons. In

In *May* at *Naples*, the wind lay much in the weſterly and ſoutherly points. Rain 103 meaſures and ½, which make 4 nches 11 meaſures and ½ in depth, with frequent thunder. *Veſuvius* caſt out rivers of fire, which reach'd almoſt to the bottom of the mountain.

At *Bengal* the winds varying, but for the moſt part ſouther-y, with much cloudy, rain and thunder. The beginning of he month colder than ordinary; afterwards exceeding hot.

In *June*, at *Naples*, the wind was much in the weſterly and north weſterly points, but little rain, only 6 meaſures and ½, which is but about ¼ of an inch depth.

At *Bengal* much rain, with thunder and heat. On *June* 5th it is noted; 'we are now pretty certain the rains are ſet in.'

At *Chriſtiana*, the obſervations begin on *June* 22, the wea-her temperate, and for the moſt part cloudy, with thunder, hail, nd rain.

At *Naples July* was a hot dry month; without any rain, but requent miſts. *Veſuvius* quiet.

At *Bengal*, there was frequent and much rain, with thunder and lightning; for the moſt part cloudy; winds perpetually varying.

At *Chriſtiana* great rains with thunder, frequent fogs, ſome air, and complaints of vehement heat; tho' the thermometer was but at 30 degrees in that month.

In *Auguſt*, at *Naples* the wind was in the weſterly and north-weſterly points, ſhowers with thunder were frequent, which amounted only to 49 meaſures and ½, which is but a little above 2 inches depth. And tho' by the table the weather ſeems to have been warm; yet there are frequent complaints of the air being cold. *Veſuvius* caſt forth a large river of fire.

At *Bengal*, much rain, with thunder and cloudy, winds va-rying, but pretty much eaſterly; weather ſometimes very hot, but for the moſt part more temperate than in ſome of the other months.

At *Chriſtiana*, the wind various; frequent miſts with clou-dy, and ſometimes fair, and ſometimes rain. Great complaints of heat, tho' no great ſigns of it by the thermometer.

In *September*, at *Naples*, the winds various and very ſtormy towards the latter end of the month, with horrible thunder, lightning, and heavy rain, which amounted to 220 meaſures and ½, making 9 inches 13 meaſures and ½ in depth; which is more than fell in any month this year, and drown'd the marſhes, and did a great deal of damage to houſes, trees, *&c.* *Veſuvius* was quiet at the beginning, but fiery at the end.

Bengal hath only the 7 first days observations, where the wind was mostly easterly, cloudy and showery, with thunder and lightning.

The observations of the remaining months are all of *Naples*; where in *October*, the wind was various, and sometimes stormy, with thunder; frequent mists, and sometimes heavy rain, amounting to 107 measures, which is 4 inches 15 measures, and in the mountains, snow. *Vesuvius* was turbulent in the beginning of the month, and emitted a river of fire.

November was, for the most part, a cloudy misling month, with thunder and rain; but of no greater quantity than 73 measures, which are equal to three inches 4 measures depth *English*. Then the wind was more northerly than in any other quarter. The fire of *Vesuvius* less.

December was a wet, seasonable month, the rain being 179 measures, which is 7 inches 18 measures in depth; and this upon the rains and unseasonable weather of the preceeding months greatly damaged the fruits of the earth. The rain of the whole year the illustrious observer computes at 3 *English* feet 7 inches and 14 ½ measures: And to show how much wetter this year was than these other, he gives these quantities of the year 1724. 2 *English* foot 10 inches, 14 measures: Of 1725, 2 foot 10 inches, 17 measures: Of 1726, 1 foot, 11 inches, 14 ½ measures

The dead bodies of a Man and Woman preserved for 49 *years in the Moors in* Derbyshire, *by Dr.* Charles Balguy. Phil. Trans. N° 434. p. 413.

THESE two persons were lost in a great storm of snow on the moors, in the parish of *Hope*, near the woodlands in *Derbyshire* on *Jan.* 14. 1674; and not being found till the 3d of *May* following, they then smelt so strong, that the coroner ordered them to be buried on the spot: They lay in the peat-moss for 28 years and 9 months before they were look'd at again; when some countrymen, having observ'd the extraordinary quality of this soil in preserving dead bodies from corruption, were curious enough to open the Ground, to see if these persons had been so preserved, and they found them no way alter'd, the colour of their skin being fair and natural, and their flesh soft, as that of persons newly dead: They were afterwards exposed for 20 years, tho' they were much changed in that time, by being so often uncover'd: And in 1716 their condition was as follows: The man was perfect, his beard strong and

and about $\frac{1}{4}$ of an inch in length; the hair of his head ſhort, his skin hard, and of the colour of tanned leather, which is nearly that of the liquor and earth they lay in: The woman being taken out of the ground, had one leg off, the fleſh decay'd, and the bone ſound; the fleſh of one hand decay'd and the bone ſound; the upper lip and the tip of her noſe decay'd, but no other part of her face: Her hair was long and ſpringy, as that of a living perſon: The upper part of one of her foreteeth, as far as was contained in the ſocket, was as elaſtic as a piece of ſteel; and wrapt round the finger, it would ſpring again to its firſt form: But it loſt this power after it had been kept for a few minutes in the pocket: They were afterwards buried in *Hope* Church, where viewing them ſome time after, they were found entirely conſumed.

They had lain about a yard deep in the ſoil or moiſt moſs, but without any water in the place: The man's legs, which had never been uncover'd till now he was buried, were quite fair; the fleſh, when preſſed with the finger, pitted a little, and the joints play'd freely, and without the leaſt ſtiffneſs: The other parts were much decay'd: What was left of their cloaths, (for the greateſt part of them had been cut away, to carry home as a curioſity) was firm and good: The woman had on a piece of new ſerge, which ſeemed never the worſe.

The ſequel of the Account of Ambergreaſe *by Dr.* Neuman. Phil. Tranſ. N°. 435. p. 417. *tranſlated from the* Latin.

AS to outward appearance, there are ſeveral ſorts of ambergreaſe, as aſh-colour'd, whitiſh, yellowiſh, of a dark aſh colour, and ſmooth, as if done over with leather, red, veiny, ſpotted, and entirely blackiſh, both that ſwallowed by animals and afterwards either ejected into the ſea, or found in their bodies, as that which was not ſwallowed. The ambergreaſe, that has been ſwallowed, is both the baſeſt and leaſt valuable; for, it generally retains ſomething of the place, from which it was taken, or ſome animal factor, and conſequently is eaſily diſtinguiſhable from good ambergreaſe: So likewiſe, that ſort, which is entirely blackiſh, or whitiſh, is of no great value: In like manner, what appears ſmooth, uniform, and too pure to the eye, is to be ſuſpected, as rarely genuine, and generally adulterated, if not entirely factitious and compounded. On the contrary, what appears aſh-colour'd, veiny, or even ſpotted white, black, or yellow, coated over with an external blackiſh cruſt;

cruſt, tho' not ſo very pure, but mixt here and there with the becks of birds, parts of the cuttle fiſh, the ſpines of fiſhes or other matters, yet for all this it is reckoned the beſt: Tho' the admixture of theſe impurities be not always ſo neceſſary, but that, if it can be had, it may be choſe rather pure than impure; the Dr. only mentions this, to ſhew that the beſt ſort is generally mix'd with variety of traſh.

The primary properties of good ambergris, beſides thoſe already mentioned, are the following; that it be light, and feel almoſt like wax, and at the ſame time be friable, yet a little tough, ſo that in pounding, it may ſtick to the mortar or peſtle, be of a fragrant ſmell, eaſily take fire at a burning candle, and conſtantly preſerve its flame, eaſily melt over the fire or a live coal, and have no particular taſte, neither bitter nor auſtere, neither acid nor ſaline.

The common way of trying the genuineneſs of ambergris, is (to prevent waſting much of it) to prick it with a needle, or other fine inſtrument, made red hot; and then ſomething like melted roſin ſhould appear about the needle and riſe up: Or it is thrown on live coals, or a little of it is melted in a ſilver ladle over a burning candle: This proof, it is true, has its uſe; but if you cannot accurately diſtinguiſh the exhaling odour, and beſides, obſerve the various other circumſtances, and only attend to its melting, even this common proof may fail, and it may equally hold in adulterated, nay in factitious ambergris; as does alſo that other recommended by *Etmuller*; *viz.* that genuine ambergris ſoftens between the fingers like wax, whereas the factitious is friable; a property that may be eaſily given it by art, and which is neither conſtant nor univerſal.

Ambergris is adulterated, nay even compounded by impoſtors, with wax, roſin, ſtorax, benjamin, amber, *ladanum*, muſk, civet, aloes, the rottenneſs of the aſh-tree, the flower of rice, the moſs of trees, *&c.* According as one cheat improves upon another, in a juſter proportion and mixture of theſe ingredients; but the trick is eaſily diſcovered.

1. The factitious and compounded ambergris appears generally quite uniform, and every where of the ſame colour and mixture, like a maſs of pills or like dough; a thing that never ſo equally holds in natural ambergris. 2. Such counterfeit ambergris generally ſoftens ſooner in a warm hand than the genuine. 3. But the beſt way of diſcovering ſuch factitious ambergris is by the ſmell; for, as the true ſmell of ambergris is entirely

tirely peculiar, ſo it cannot be ſo eaſily imitated by art, but that this or the other ſmell from the ingredients ſhall ſometimes predominate, and ſo the cheat be diſcovered; and that the more eaſily, if you ſtrew it on live coals, or melt it in a ſilver ladle over a burning candle, or even a bit of it be held in the naked flame, and you ſnuff its fume, and alſo attend to its melting and taking fire: If it be genuine, it will then boil in melting, continue brown after melting, and at laſt diffuſe an amber odour, tho' weaker than common amber: But if it be counterfeit, it will fail in theſe properties more or leſs: For, this latter will either not melt at all, or later or much ſooner: In boiling and in its odour it will be different; as it will either yield a ſomething fetid, or more fragrant ſmell, or an entirely different ſmell from the fumes of true ambergris: And then after toaſting and boiling, it will not continue of the ſame appearance as true ambergris, but be like a coal, aſhes or earth, and conſequently exhibit a quite different appearance, even in reſpect of colour. 4. Should ſuch adulterated ambergris occur as had no foreign ſmell, and which notwithſtanding happened to be compounded with ſomething inodorous, as the moſs of trees, and conſequently ſhould have no other ſcent than that of true ambergris, even ſuch a cheat ſhall be eaſily diſcover'd: For, ambergris in this proportion ſhall ſmell too ſparingly or weaker; and thrown on live coals it ſhall fume more copiouſly, if not ſenſibly ſtink; nor will it melt equally all over, nor in melting, boil as the genuine ſort does; in fine, it ſhall not be ſo eaſily kindled in the flame of a candle, nor burn inceſſantly till it is ſpent like a ſmall candle, which yet genuine ambergris does. 5. Moreover, all factitious or adulterated ambergris in ſolution ſhall appear quite different from the genuine. 6. And laſtly, in diſtillation in an open fire the counterfeit differs greatly; not to mention other proofs at preſent.

Genuine ambergris, therefore, is a bitumen, and among all the known ſpecies of bitumens it equals or comes neareſt to amber, excepting in its hardneſs and tranſparency: It comes neareſt to it in its compoſition, not only beyond the other bitumens, and all other minerals, but beyond all other things in nature of what denomination ſoever they may be, as ſhall appear farther on.

Tho' the term bitumen denote nothing other than ſome mineral mixt, yet moſt writers of natural hiſtory have confined it to this ſpecific meaning, that ſuch a mixt is properly a

clammy

clammy fat of the earth, which eaſily burns; and this character perfectly agrees with ambergris: For, a fat or oil conſtitutes the baſis or bulk of its eſſential parts, and one drachm thereof holds at leaſt 2 ſcruples and $\frac{1}{2}$ or $\frac{5}{6}$ parts of oil; on which account ambergris may not improperly be called a *fat*: But as this fat or oil in ambergris is neither a vegetable nor animal, but plainly a mineral oil, it may hence with the greateſt propriety be called *a fat of the earth*: That ambergris is a clammy body or clammy fat in reſpect of other fat cannot be queſtioned, otherwiſe it might be eaſily ground and powdered, and not ſtick in ſuch a manner to the peſtle and mortar: In fine, that it readily takes flame, may be eaſily ſhewn, if you apply a bit of ambergris to a burning candle; conſequently it has all the requiſites and characteriſtics of a bitumen.

That ambergris approaches neareſt to amber of any ſubject, Dr. *Neuman* had learned by experience: For, 1. Ambergris by itſelf, and in a dry form, melted over a naked fire, conſequently gradually exhaling, and alſo on a live coal, has a ſtrong amber ſcent; but if ambergris be with water committed to the fire, it will then in like manner melt like roſin, and give the water in ſome meaſure an amber flavour, tho' it does not mix with it; and this flavour is alſo eaſily taken off by diſtillation; but the ſurrounding moiſture prevents both the reſolution of the mixt and the evaporating of ſome eſſential particles; in a word, the amber ſmell does not this way ſo diſcover itſelf, as when it is melted dry. 2. Ambergris, in the preparation of varniſh, differs not from amber; if ambergris be melted, and dephlegmated lintſeed-oil be afterwards poured to it, or other ſomewhat oleaginous varniſhes be mixed with it. 3. And laſtly, the moſt convincing experiment is the diſtillation of ambergris in an open fire; for, in this proceſs every circumſtance perfectly and nearly coincides as in that for amber: Dr. *Herman Nicolaus Grimm*, who had been for a long time in the *Eaſt-Indies*, gradually diſtilled in a glaſs retort in an open fire an ounce of ambergris; and there came over firſt, a phlegm, then a ſpirit, and then a yellow oil and a little volatile ſalt; and there remained in the retort ſomething like pitch; and he plainly tells us, that the phlegm, oil, ſalt and remainder, had the ſame appearance, the ſame properties, as theſe very parts have in the diſtillation of amber, only that the oil diffuſed a ſomewhat more fragrant ſmell, *Ephemer. Nat. Curioſ. Dec.* II. *Ann.* I. p. 405.

p. 405. Dr. *Neuman* imitated this experiment on a drachm of ambergris, and found the very ſame things, with this only difference, that in his proceſs, he had got no conſiſtent remainder, but a little powder not exceeding a grain in weight; which neither affects the thing itſelf, nor Dr. *Grimm*'s experiment; but the difference was probably owing to this, that he finiſhed his proceſs a little ſooner, or at the laſt did not apply a ſufficiently ſtrong fire; for, in this caſe we obtain ſuch a remainder; and conſequently Dr. *Neuman* found that the whole body of ambergris may in a conſtant, violent degree of fire be diſtilled and brought over; and moreover, he alſo obtained from a drachm of ambergris 2 ſcruples and ½ of oil, 5 *gr.* of water, 2 *gr.* of ſalt, and about a grain weight of powder; the other two grains were loſt by partly adhering to the ſides of the retort and by partly evaporating: The oil and ſalt, as being the two principal parts, were of the ſame nature, as thoſe of amber, conſequently, not urino-volatile: From this proportion of its ingredients we may reflect that ſo ſmall a quantity of ſalt and earth is however capable of condenſing a much greater portion of oleaginous parts, or of reducing them to a firm, tenacious and entirely dry ſtate; alſo, that this circumſtance in a greater meaſure agrees with amber, and conſequently confirms its affinity even in this reſpect: All ſpecies of ambergris do not ſeparate any pure ſalt, ſince that little it has is eaſily intangled in a copious oil.

Since, therefore, this precious matter, ambergris, exerts its principal virtue by its fragrancy; hence it has been uſed for a long time in perfuming; as in balſams, ſnuffs, dentifrices and electuaries for the teeth, powder for the hair, waſh-balls and in perfuming clothes; and conſequently, rather applied for delicacy than uſefulneſs: Since likewiſe a variety of medicinal qualities was formerly aſcribed to ambergris, as a cardiac, cephalic, apoplectic, bezoardic, nervine, *&c.* Hence it was uſed in the ſhops, tho' formerly in greater plenty than now, in different preparations and compoſitions, yet more in the galenical than chemical ones, and commonly in the form of a powder. But to wave theſe, and mention one only inſtance of a very common preparation ſtill in uſe; *viz.* the ſimple eſſence of ambergris; in which there is nothing other than pure ambergris with its menſtruum; and the rather as in the ſolution of it, he obſerved one or two circumſtances, not to be met with in any author.

It has hitherto been received as a certain truth, that rectified ſpirit of wine does not of itſelf diſſolve ambergris; and from this authors concluded that ambergris was neither vegetable nor animal; neither a roſin, nor an oleaginous fatty or reſinous body, but a bituminous mineral; becauſe rectified ſpirit of wine affects it very little, much leſs diſſolves it; and only in the ſame manner as it affects aſphaltum, amber and other bituminous mixts; from which indeed it may extract a little, but never make a perfect ſolution: And even Dr. *Hoffman* himſelf, *Obſervat. Phyſic. Chym. Select. Lib. I. Obſerv. XVIII. p.* 67, *&c.* affirms that 'the ſolution and extraction of all reſinous bodies are eaſily performable in highly rectified ſpirit of wine; but, which is remarkable, he ſays, that this does not hold in ambergris, which is of very difficult ſolution in this ſpirit: And becauſe we obſerve that inflammable bodies, which grow out of the earth, as amber, jewiſh pitch, pit-coal, are alſo of more difficult ſolution, nor ſo readily mix with a highly ſpirituous liquor, hence it is we give in to thoſe, who hold that ambergris is referable to bitumens, whoſe origin is from the earth, *&c.*' And further he ſays; 'ſince ambergris therefore is of ſo difficult a ſolution, this undoubtedly is the reaſon that we have no genuine ſolution of it in the ſhops: For, generally they are wont to prepare it with muſk, the eſſential oils of cinnamon and other oils, or even with civet; and thus indeed we are poſſeſſed of a very fragrant eſſence, and which is not without its virtue and uſe; but it partakes but little of the ambergris, which latter remains rather untouch'd.' But Dr. *Neuman* has from his experience ſhewn this ſolution in ſpirit of wine, not only poſſible but eaſy, and it requires only ſome little management: Take highly rectified tartariſed ſpirit of wine (tho' in caſe of neceſſity, the ſimple rectified ſpirit without tartar may equally anſwer) and put to it $\frac{1}{12}$ part of pure, genuine ambergris cut ſmall; put it in ſuch digeſtion, as gradually heats, ſo that the ſpirit or menſtruum begin to boil; and this is all the art: Then a perfect ſolution enſues; for, he diſſolved 2 ſcruples of ambergris in an ounce of ſpirits, and there remained undiſſolved in the bottom of the glaſs, that impurity only, which was not ambergris, but conſiſts of earthy and other heterogeneous parts, ſometimes 2 grains, or only one grain in weight: For an extemporaneous proof of this; ſet a glaſs, which is not quite filled, nor cloſely ſtopped, for fear of its burſting, over live coals

coals or a burning candle; and the experiment ſucceeds, as ſoon as the ſpirits begin to boil: But if we uſe an inflammable oleaginous ſpirit as the menſtruum, whether drawn over one or more oleaginous vegetable, or whether ſome eſſential oil be dropped into ſuch highly rectified ſpirit of wine, then the ſolution ſucceeds the ſooner; and for the reſt, neither ſpirit of roſes, nor any other ſpirit, which is drawn over, unleſs it be rich in oil, or impregnated with oleaginous particles, has here, with reſpect to the power of the ſolvent, any preference to highly rectified ſpirit of wine.

The following circumſtances are remarkable. 1. That if highly rectified, ſimple ſpirit of wine without *alcali*, or even tartariſed ſpirit of wine, newly drawn over ſalt of tartar, be taken; then indeed in the aforeſaid proportion there is alſo a compleat ſolution; but this ſolution or eſſence of ambergris, as it is called, is not ſufficiently deep or high coloured. 2. The remainder is apparently no inconſiderable quantity, ſo that the greateſt, or a conſiderable part of the ambergris appears indiſſolved; tho' really, upon filtring the ſolution, and drying and weighing what remains in the filtre, it prove very little in quantity, and is a pure powder or tender earth, provided the genuine ambergris be uſed. 3. If the ſpirit be not ſufficiently dephlegmated, or a ſufficient degree of heat be not applied; in that caſe, there is either no ſolution at all, or a bare and ſimple extraction only. 4. But if you uſe for the ſolution ſuch tartariſed ſpirit of wine, as has only ſtood in infuſion with ſalt of tartar, without afterwards drawing it over, then the ſolution is ſooner and better tinged.

From theſe four circumſtances, therefore, ariſe the following conſectaries. 1. Since generally in the eſſence of ambergris, if its colour or tincture is alſo ſought for, many perhaps have not taken it for a ſolution, becauſe it appeared but little tinged. 2. And that the leſs, as they obſerved ſo apparently copious remains; which at firſt made the Dr. himſelf think, there was little or no ſolution, till he had dried and weighed it, when he found it not only very ſmall in quantity, but a pure heterogeneous powder, that made no part of the ſolution or eſſence of ambergris. 3. Or if this ſolution has not ſucceeded to any one's mind; in that caſe, the ſpirits were not ſufficiently rectified, or a due degree of heat was not applied; and probably many have thought, that as ambergris was a fragrant matter, if either digeſted or infuſed in a ſtrong

heat, its beſt and moſt ſubtile parts would evaporate, and therefore a gentle degree of heat was to be applied; tho' indeed all ſorts of ambergris do not require ſuch a vehement fire, and yet the ſolution ſucceed. 4. And laſtly, it follows, that if you either obtain or have a highly tinctured eſſence, and beſides the menſtruum, you uſe nothing other than ambergris, ſuch menſtruum is always a ſpirit of wine tartariſed by infuſion; whence the tincture originally proceeds, not from the ambergris, but from the fixt alcaline ſalt, and is only heightened by the oleaginous parts of the ambergris: The certainty of this circumſtance appears, if highly rectified, oleaginous ſpirit, of wine, be digeſted *per ſe* with ſalt of tartar only; for, hence ariſes a ruddy tincture, called tincture of tartar: If this ſpirit of wine be not oleaginous; neither the tincture of tartar will be ſo beautiful, and ſcarce be tinged yellow, or it will be entirely without any tincture; but if you put to it a drop or two of eſſential oil, as of *aniſe*, you will plainly ſee a more ſaturated tincture.

The concluſion of this matter is, therefore, this: 1. That if ambergris be treated with pure and highly rectified ſpirit of wine, or even with ſuch as is tartariſed by drawing it over; then indeed there is a compleat ſolution, but without any peculiar tincture. 2. But if the ſame ambergris be treated in the ſame manner with ſpirit of wine tartariſed by infuſion; that then there is not only a ſolution, but alſo a tincture, which is chiefly owing to the ſalt of tartar, and conſequently in reſpect of colour is rather to be conſider'd as a tincture of tartar. 3. But if it appears a little higher coloured than tincture of tartar, in that caſe the oily parts of the amber have in like manner contributed to the heightening the tincture, as if you had put one or more drops of another oil to another tincture of tartar, to which no ambergris was uſed. 4. If, therefore, the alcaliſate ſpirit of roſes, made either by fermentation or even by repeatedly drawing it over roſes, be applied as a ſolvent in the preparation of eſſence of ambergris, and with it an excellent eſſence be prepared, we are not hence to conclude, as if ſpirit of roſes alone were the peculiar and appropriated ſolvent of ambergris: For here, as was already ſaid, there is ſome alcaline ſalt, as alſo ſome oleaginous parts; and from theſe the tincture ariſes in the ſame manner, as if tincture of tartar were prepared from ſalt of tartar with oleaginous ſpirit of wine, without ambergris and without roſes; and the ſolution of ambergris always

always proceeds, as if it were a ſolution with another tartariſed ſpirit of wine, or even with what is highly rectified and ſimple, without ſalt of tartar.

But that the eſſence or ſolution of ambergreaſe, prepared in a proper manner with good ſpirit of roſes, diffuſes a much ſtronger and more grateful odour, than that prepared with ſimple ſpirit of wine, is entirely natural. For, 1. Spirit of roſes *per ſe*, has not only a ſtrong but a fragrant odour, whereas the ſimple ſpirit of wine has almoſt none at all: 2. It is known that ambergreaſe is naturally diſpoſed not to yield any peculiar odour; but ſo ſoon as ſome other thing, naturally fragrant is mixed therewith; that then the odour and fragrancy of ambergreaſe is, as it were, rouſed, excited, and really exalted; and this is the caſe in the ſolution of ambergreaſe with ſpirits of roſes.

The officinal eſſence of ambergreaſe may, therefore, not improperly be prepared with alcaliſate ſpirit of roſes on the account of heightening the odour; but as there are many, who cannot bear either the ſmell or taſte of roſes, hence it will be proper to prepare this eſſence with highly rectified and alcaliſate ſpirit of wine: But that this tincture may be ſufficiently efficacious, and the ſolution the ſooner accompliſh'd, and not without its colour, it will be proper doubly to alcaliſate the ſolvent or ſpirit of wine; as firſt prepare a pretty good tartariſed ſpirit of wine by drawing it over, or let a genuine highly rectified ſpirit of wine be ſeveral times diſtill'd on a fixt alcaline ſalt; and let it be again returned on pure and calcined fix'd alcaline ſalt, and digeſted for ſome time; at length decant it, and uſe it as a ſolvent for the eſſence or ſolution of ambergreaſe; and then this ſpirit, in reſpect of its ſolutive virtue, will act in the ſame manner as the beſt and moſt precious ſpirit of roſes; nay if the ſpirit of roſes was neither tartariſed nor alcaliſate, it will render the preference doubtful.

Etmuller ſomewhere writes, *opera Pharm. Chym. in Schrodero dilucid. p.* 79. that 'ambergreaſe is made to fer-'ment with roſes, and hence the *ſpiritus roſarum ambratus* 'may be made, which is to be preferred to pearls, as the 'greateſt cardiac and comforter': But ſince water, as the common ſolvent of bodies that are to be fermented, does not in the leaſt touch ambergreaſe; and that a true fermentation cannot poſſibly happen to roſes on the part of ambergreaſe; it is probable this is a miſtake of *Etmuller*'s; into which he was led by being told, that a grateful ſpirit is made

made with ambergreaſe and ſpirits of roſes well fermented and rectified: That the fragrant ſpirit of roſes, even without ambergris, is preferable to pearls, only on account of its fragrancy, is not to be doubted; for, as a cardiac Dr. *Neuman* prefers crabs-eyes, nay common oiſter-ſhells to pearls; but this by the bye.

It now only remains to prove that an inflammable oleaginous ſpirit promotes the ſolution of ambergreaſe, as may be ſhewn to the eye: Take highly rectified inflammable ſpirit, and throw into it bits of ambergris; if you obſerve them not to diſſolve, then pour in a few drops only of any pure, eſſential, diſtilled, oil, free of all expreſſed oil, and the experiment manifeſtly ſucceeds in a ſhort time: The reaſon is, that ſuch oils are ſolvents of ambergreaſe; ſince the experiment always ſucceeded not only with the various aromatic, fragrant, eſſential oils, as with that of mint, lavender, *&c.* but with the *Italian Oglio di cedro*, with the reſinous oil of terpentine, as alſo with the pure, rectified, bituminous, oil of amber, which is of the ſame family with itſelf, whereas with expreſſed oil, as that of almonds there was not the leaſt ſolution or extraction; from which it appears the more evidently that *Schroder*'s eſſence of ambergreaſe, by firſt digeſting and expreſſing the ambergreaſe with oil of almonds, and afterwards drawing highly rectified ſpirit of wine over what is thus expreſſed, rather hinders than promotes a ſolution.

Dr. *Neuman*, with a view to the ſolution of ambergreaſe, made trials with dulcified ſpirits, both alcaline and acid; and he infuſed and digeſted it with the dulcified ſpirit of vitriol, nitre, and ſalt; as alſo with dulcified alcaline ſpirits, as of urine, or the vinous ſpirit of ſal-ammoniac, as it is called, with quick lime, and prepared ſalt of tartar; but theſe extracted little or nothing, and would not entirely diſſolve it: In the infuſion with dulcified ſpirit of vitriol there happened ſomething ſingular; this ſpirit, with the little it extracted, form'd ſome ſaline grains, which ſettled to the ſide of the glaſs, wherein the infuſion was made.

That white viſcid matter, of the appearance of tallow, which is commonly wont to precipitate from the ſolution or eſſence of ambergreaſe; M. *Lemery* takes to be wax; and from this circumſtance he would prove, that ambergreaſe is nothing other than wax.

But if this white matter happen to ſeparate, then in that caſe the Dr. commonly obſerv'd the three following circumſtances about

about it, which if they are not conjointly abſolutely requiſite to the precipitation, yet at leaſt one of them is ſo for either the eſſence or ſolution as ſtood in a glaſs that was not quite full, but half or a third part empty; or the glaſs has not been ſtopt ſo tight but that the moſt ſubtle ſpirits have inſenſibly exhaled; and therefore in proportion to that exhalation a little of the ſolution is again precipitated; laſtly, the ſolution has ſtill ſtood over ſome part of the indiſſolved ambergreaſe. For if the ſolution be immediately decanted from its remainder, and a glaſs be quite fill'd up with it, and ſtopt very tight, to prevent its evaporating, then none of it precipitates ſo ſoon, nor does any ſuch whitiſh matter appear; conſequently the evaporation of this very ſubtile ſpirit is the true and principal cauſe, that it again precipitates in its flight what it held before diſſolved.

2. The whitiſh matter which M. *Lemery* takes for wax, is nothing other than a ſort of depurated, or reduced perfect ambergreaſe; which appears from treating the whitiſh matter in the ſame manner as the ambergreaſe was before with highly rectified ſpirits of wine or any other ſolvent made uſe of for the latter; whereas if the trial be made with wax, and highly rectified ſpirits of wine, they ſhall not diſſolve the wax ſo readily, nor incorporate with it in the ſame manner. Not to mention other circumſtances at preſent.

An Account of Experiments made on Ambergreaſe; *by Mr.* John Browne, *and M.* Godfrey Hanckewitz, Phil. Tranſ. N° 435. p. 437. Tranſlated from the Latin.

THE Royal Society ordered Mr. *Brown* and M. *Hanckewitz* carefully to repeat Dr. *Neuman*'s proceſs on ambergreaſe. Mr. *Browne* judging that the Dr. had in his experiment made uſe of too ſmall a quantity of ambergreaſe, namely, a ſingle drachm, took an ounce and an half, together with very dry white cimolian earth, (which he always makes uſe of for extracting the ſalt from amber) reduced to a powder, and putting them into a retort he expoſed them to different degrees of heat; he firſt obtain'd a pellucid phlegm like the pureſt water, then a deep ſpirit like beer, after that there came over an oil, of a deeper colour, and laſt of all, with a very ſtrong fire, he obtain'd a thick black balſam. The oil and balſam he owns, had the ſame ſmell with thoſe got from amber, but without any acid volatile ſalt, nor did the ſpirit from ambergreaſe ferment with alkali's, as that from amber does which abounds with ſuch an acid ſalt: He takes that volatile acid

acid to be the peculiar criterion of amber. The remains of amber after diſtillation, are hard, black, and like jet; but after diſtilling the ambergreaſe, the earth only remained tinged black; and therefore ſince he could not perceive any acidity or volatility throughout the whole proceſs, he leaves it undetermined, whether ambergreaſe be an animal excrement or not; but he obſerves that all its grateful odour, is entirely loſt in the gentleſt fire.

M. *Hanckewitz* diſtill'd in a retort 2 ounces of ambergris, mixt with double the quantity of very pure white ſand; and after that, he diſtilled in like manner other 2 ounces, and in each proceſs he got a limpid oil, and the remainder was bituminous: The oil rectified *per ſe* yielded a phlegm of a grateful ſubacid taſte, like a weaker vinegar, and after that there came over a limpid, balſamick, bituminous oil, like petrol: He diſtill'd half an ounce of ambergreaſe *per ſe*, and with a pretty moderate fire he obtain'd the ſame things. After diſtilling the ambergreaſe to the greateſt degree of dryneſs, he urged the remainder with the ſtrongeſt fire, and at laſt there remained 3 grains of a white ſaline earth, that cauſes a moderate effervesſence with acids, or runs *per deliquium* when expoſed to a moiſt air, ſince he could obtain no volatile ſalt, nor phoſphorus from the coal or blackiſh remainder of the 2 firſt proceſſes, he concludes with the greateſt aſſurance, that ambergreaſe is no animal ſubſtance, or excrement: For, phoſphorus may be got from the excrements of all known animals, as he has abundantly ſhewn in *Phil. Tranſ.* N° 428. He therefore takes ambergreaſe for a bitumen, nearly approaching to the nature of amber; but not for a genuine amber; ſince it does not yield an acid volatile ſalt, like that got from amber.

He again repeated the experiment with equal parts of ambergreaſe and powder'd glaſs; becauſe there might ariſe a ſuſpicion of ſome alcaline earth adhering to the ſand, that might have abſorbed whatever quantity of acid were got from the ambergreaſe; but the proceſs exhibited again the ſame things, only that the phlegm had the taſte of a neutral, but not an acid ſalt; and after the fire had fus'd the glaſs powder, the bituminous remainder free from the maſs of glaſs, lay upon it like a black coal, and ſpread itſelf over all the inſide of the retort up to the very neck, like fine and ſhining black flowers or flakes.

All controverſy about theſe experiments may be eaſily determined, if we conſider ambergreaſe as a mixt conſiſting of

ſeveral

PLATE III.

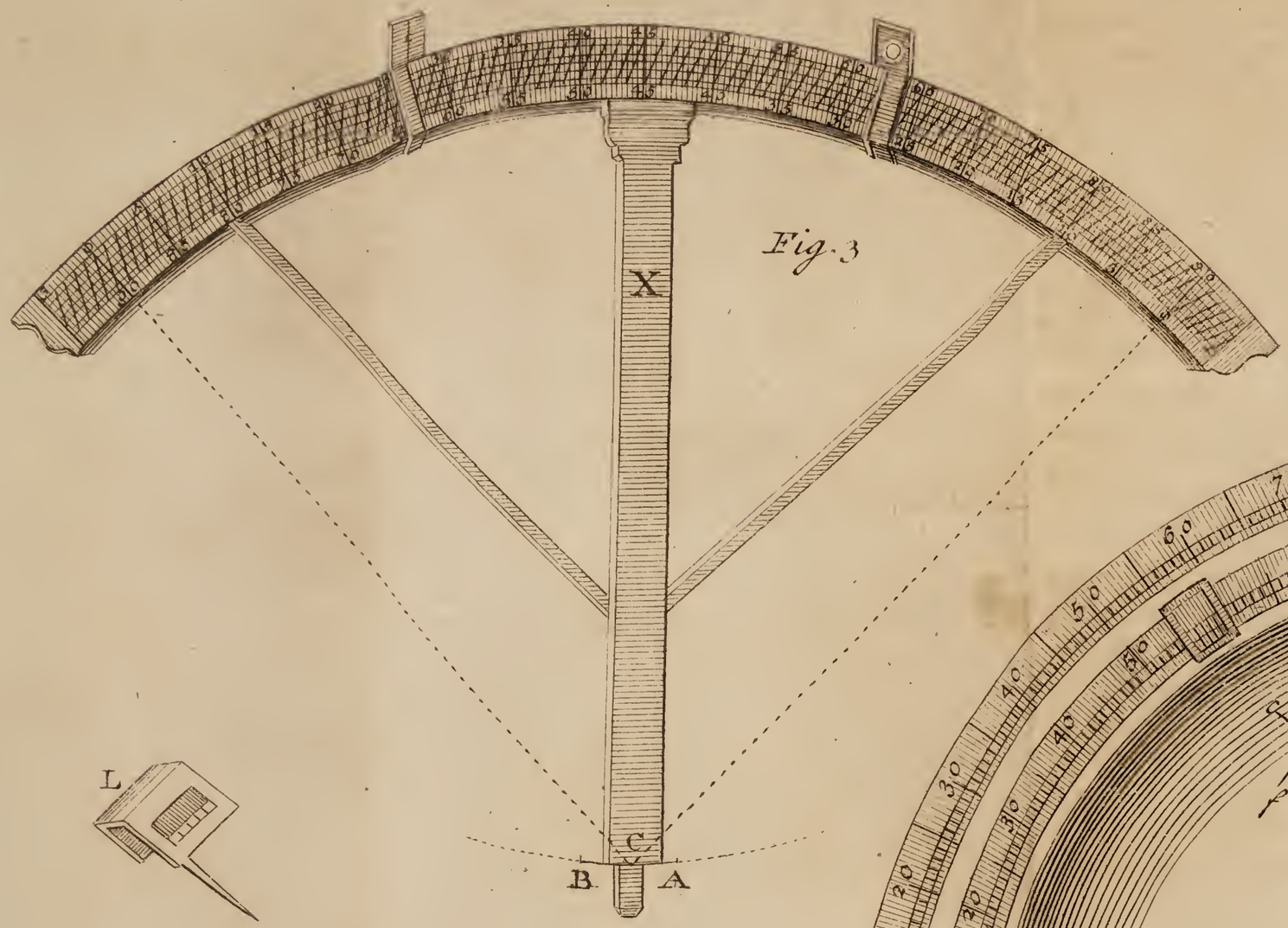


Fig. 3

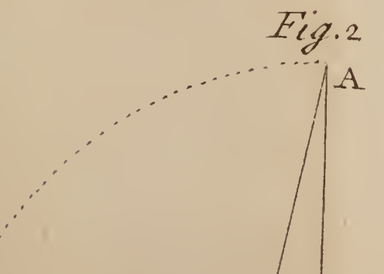
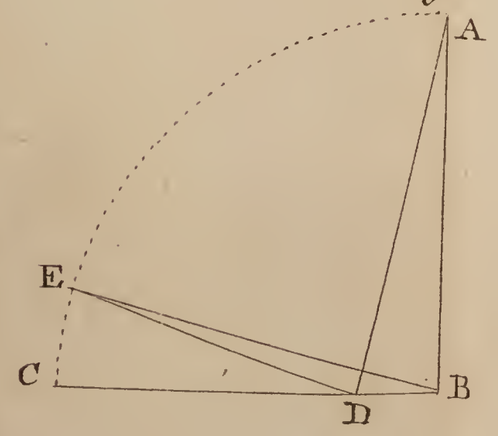
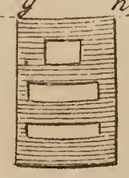


Fig. 2





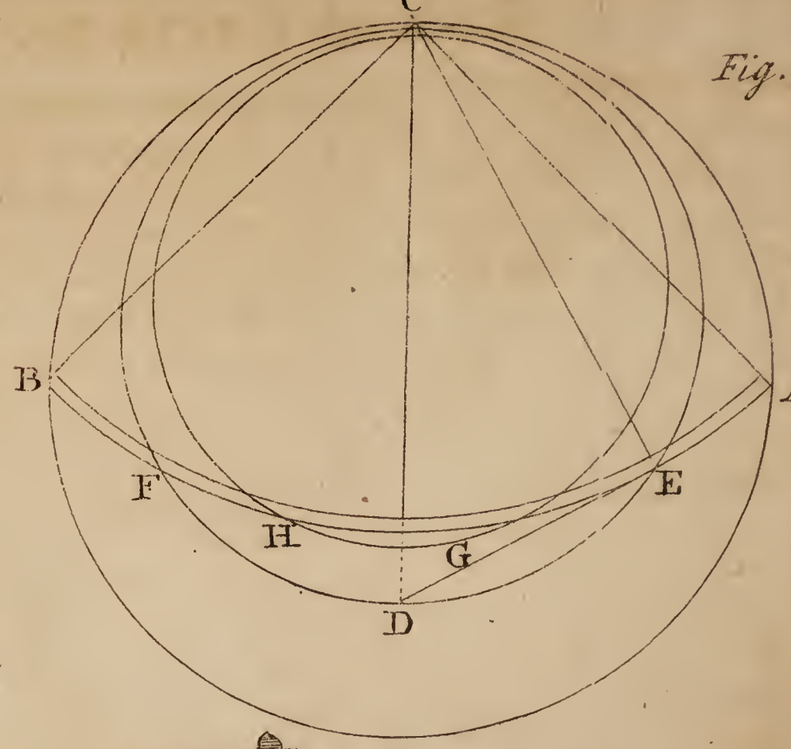


Fig. 3

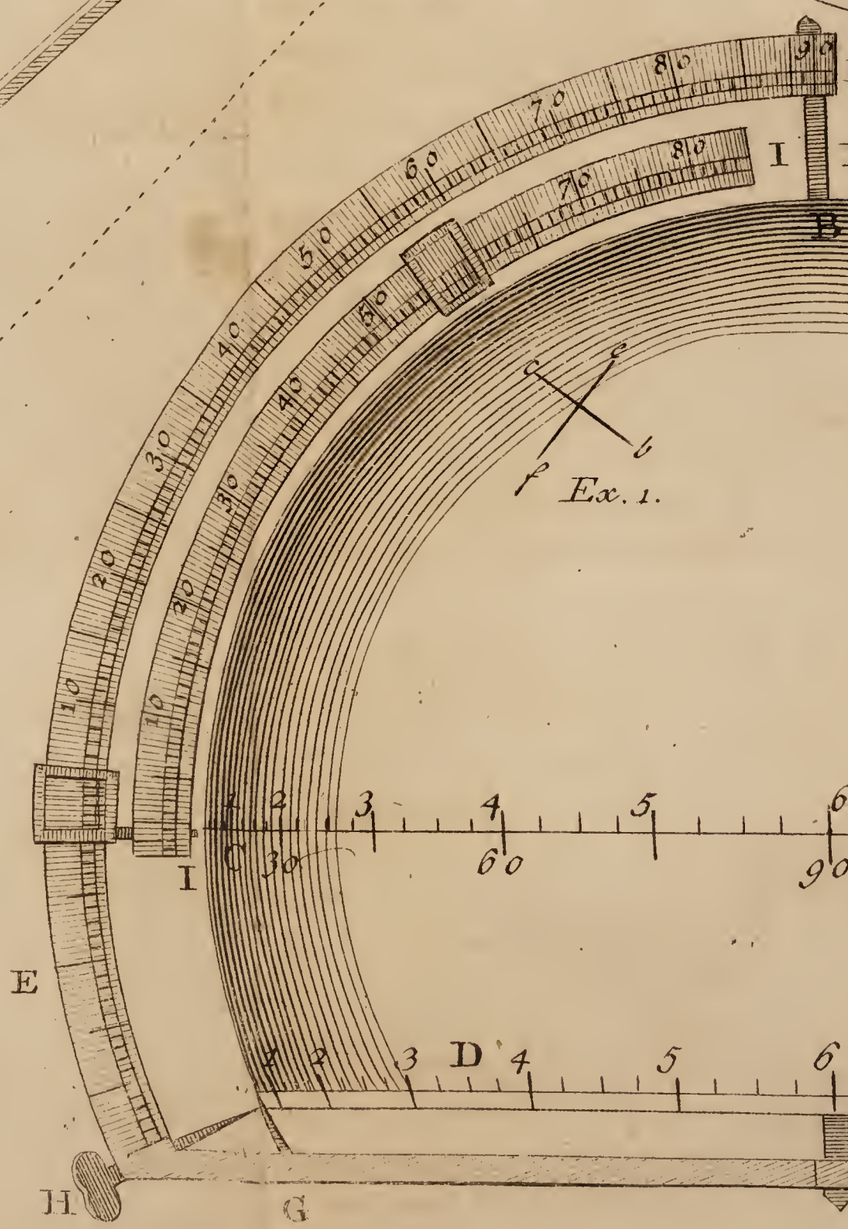


Fig. 4

ſeveral adventitious matters, like other minerals, and not as a ſimple pure body like metals: For no ore of any metal, as lead, for inſtance, doth in all its parts exhibit an equal quantity of metal, or particular mixt mineral; in like manner the various parts of ambergreaſe do not contain the ſame quantity of that acid ſalt; as may be ſeen in M. *Godfrey*'s experiments, in one of which the phlegm he got had a ſubacid taſte, the undoubted ſign of that ſalt; and in another it had the taſte of a neutral ſalt; and that ſmall portion of it, examined by Dr. *Neuman*, contained more of that ſalt than any other. Beſides, the more that ſalt is entangled in the oil, the more difficultly is it ſeparated therefrom. In the ſame manner it happened in ſome experiments on quick lime, according to Dr. *Neuman* in a letter to M. *Godfrey*, where he writes; that they ſucceeded differently in *England* and in *France*; whence it happened that a certain *Frenchman*, upon repeating the ſame experiments, that had ſucceeded in a certain manner in *England*, and finding that they did not anſwer in the ſame manner in *France*, affirmed that they were entirely falſe. Beſides, Dr. *Neuman*, in another letter to Sir *Hans Sloane*, ſays, that he would not be underſtood as meaning p. 421. that ambergreaſe is really amber, but only a genus thereof, or a bitumen nearly approaching to amber, which was the reaſon that the antients called both *ambar*, the one *citrina*, and the other *odorifera*. Again he adds, as to the acid volatile ſalt, of which he obtained a grain or two, that he could not be deceived; for, it diſſolved in water as common ſalt does, and it tinged ſyrup of violets of a red colour, as other acids do; and is volatile, becauſe it aſcends in Diſtillation.

An Account of Mr. Thomas Godfrey's *Improvement of* Davis's *Quadrant, transferred to the Mariner's Bow, by Mr.* Logan. Phil. Tranſ. N° 435. p. 441.

THOMAS GODFREY of *Philadelphia*, having, under the greateſt diſadvantages, made himſelf maſter of the principles of aſtronomy and opticks, as well as other parts of mathematical ſcience, applied his thoughts to conſider the inſtruments made uſe of in that momentous part of buſineſs, navigation. He ſaw that on the knowledge of the latitude and longitude of the place a ſhip is in, the lives of thouſands of uſeful ſubjects, as well as valuable cargoes, continually depend; that for finding the firſt of theſe, certain and eaſy methods are furniſhed by nature, if obſervation be duly made:

But *Davis*'s quadrant, the inſtrument generally made uſe of by *Britiſh* Navigators (tho' ſeldom by foreigners) he perceived was attended with this inconveniency, that the obſerver muſt bring the ſhade or ſpot of light from the ſun, and the rays from the horizon, to coincide exactly on the ſiducial edge of the horizontal vane: That tho' this can be done in moderate weather, and ſeas, with a clear sky, and when the ſun is not too high, without any great difficulty; yet in other caſes it requires more accuracy than can in ſome junctures poſſibly be applied, and more time than can be allowed for it. In *European* latitudes, or to thoſe nearer the northern tropick, when the ſun is in the ſouthern ſigns, and near the meridian, he riſes and falls but ſlowly: Yet in voyages to the *Eaſt* and *Weſt-Indies*, of which a great many, eſpecially to the latter, are made, he is at noon, often and for many days together, in or near the zenith; and when approaching to or leaving it, he riſes and falls, when he has declination, faſter than even at the horizon: For, it is well known to perſons acquainted with the ſphere, that when his diurnal courſe takes the zenith, he there riſes and falls a whole degree or 60 minutes in the ſpace of 4 minutes of time: So that the obſerver has but one minute, to come within 15 minutes of the truth in his latitude: While in a middle altitude, as 45 degrees he is at noon above 5 minutes and $\frac{1}{2}$ in time, in riſing or falling one ſingle minute of ſpace, the odds between which is more than 80 to 1. And yet, perhaps, no parts of the world require more exactneſs in taking the latitude than is neceſſary in voyages to the *Weſt-Indies*: For, it is owing to the difficulty of it, that veſſels have ſo frequently miſſed the iſland of *Barbadoes*, and when got to the leeward of it have been obliged to run down a thouſand miles farther to *Jamaica*; from whence they can ſcarce work up again in the ſpace of ſeveral weeks, againſt the conſtant trade winds; and therefore, generally decline to try for, or attempt it.

But farther, as the latitude cannot be found by any other method that our mariners are generally acquainted with, than by the ſun or a ſtar on the meridian: In a cloudy sky, when the ſun can but now and then be ſeen, and that only between the openings of the clouds for very ſhort intervals, which thoſe who uſe the ſea know frequently happens: As alſo in high tempeſtuous ſeas, when tho' the ſun ſhould appear, the obſerver can ſcarce by any means hold his feet; it would certainly be of vaſt advantage to have an inſtrument, by which an obſervation could alſo be ſnatch'd, as it were, or taken in much leſs

time

time than is generally required in the uſe of the common quadrant.

Thomas Godfrey, therefore, conſidering this, applied himſelf to find out ſome contrivance, by which the neceſſity of bringing the rays from the ſun and thoſe from the horizon to coincide (which is the moſt difficult part of the work) on one particular point or line from the center, might be remov'd. In order to which he conſider'd, that by the 21. 3. *Eucl. Elem.* all angles at the periphery of a circle, ſubtended by the ſame ſegment within it are equal on whatever part of the circumference the angular point falls; and therefore, if inſtead of a quadrant, a ſemicircle were graduated into 90 degrees only, accounting every two degrees but one; this would effectually anſwer: For then, if an arch of the ſame circle were placed at the end of the diameter of the inſtrument, every part of that oppoſite arch would equally ſerve for taking the coincidence of the rays abovementioned: But ſuch an inſtrument would manifeſtly be attended with great inconveniencies; for, it would in great altitudes be much more unmanageable, and the vanes could not be framed to ſtand, as they always ought, perpendicular to the rays: He, therefore, farther reſolv'd to try whether a curve could not be found to be placed at the centre of the quadrant, which would, at leaſt for a length ſufficient to catch the coincidence of the rays, with eaſe fully anſwer the intention.

A curve, that in all its parts would, in geometrical ſtrictneſs, effect this, cannot be in nature, any more than that one and the ſame point can be found for a centre to different circles, which are not concentric: It is certain that every arch on the limb may have a circle that will paſs through the centre, and be a locus or geometrical place for the angle made by that arch to fall on; but then every arch has a different one from all others, as in fig. 1. Plate III. Let A B C be the quadrant, and A B, E F, G H be taken as arches of it; circles, drawn through each two of theſe reſpectively, and through the centre C as a third point, will manifeſtly be ſuch loci or places: For, every pair of theſe points ſtand in a ſegment of their own circle, as well as on a ſegment of the quadrant, and therefore by the cited 21. 3d *Elem.* the angles ſtanding on theſe firſt ſegments will every where be equal at the periphery of their reſpective circles, and their radius will always be equal to half the ſecant of half the arch on the quadrant: For, in the circle C E D F, for inſtance, the angle C E D is right, becauſe in a ſemicircle, C E is the radius of the quadrant, E D the tangent

of the angle D C E = ½ the arch E F, and C D is the ſecant of the ſame, equal to the diameter of the circle C E D F, and therefore its radius is half that ſecant.

Now from the figure it is plain, that in very ſmall arches, the radius of their circular place will be half the radius of the quadrant, *i. e.* putting this radius = 10, the other will be 5: And the radius for the arch of 90, the higheſt to be uſed on the quadrant, will be the ſquare root of half the ſquare of the radius = ſine of 45° = 7. 071, and the arches at the centre, drawn by theſe two radii, are the extremes, the medium of which is 6.0355: And if a circular arch be drawn with this radius $\frac{1}{20}$ part of the length of it, *i. e.* in an inſtrument of 20 inches radius, the length of one inch on each ſide of the centre affording two inches in the whole, to catch the coincidence of the rays on, which muſt be own'd is abundantly ſufficient, the error at the greateſt variation of the arches, and at the extremity of theſe two inches, will not much exceed one minute.

But in fixing the curvature or radius of this central arch, ſomething farther than a medium between the extreams in the radius is to be conſider'd: For, in ſmall arches the variation is very ſmall, but in greater, it equally increaſes, as in fig. 2. where it appears, the difference between the angles A B C and A D C is much greater than the difference between E B C and E D C, tho' both are ſubtended by the ſame line B D: For, their differences are the angles B A D, and B E D. Therefore, this inequality was likewiſe to be conſider'd; and compounding both together, *Thomas Godfrey* pitch'd on the ratio of 7 to 11 for the radius of the curve to the radius of the inſtrument, which is 6. 3636 to 10. But upon farther conſideration he now concludes on 6 $\frac{4}{10}$; and a curve of this radius of an inch on each ſide of the centre to an inſtrument of 20 inches radius, or of $\frac{1}{20}$ of the radius whatever it be, will in no caſe whatever, as he has himſelf carefully computed it, produce an error of above 57 ſeconds; and it is very well known that navigators do (as they very ſafely may) in their voyages entirely ſlight a difference of one minute in latitude.

This radius is the true one for the circular place to an arch of 77° 15′, and the variation from it is nearly as great at 90 degrees as at any arch below it; the greateſt below being at about 44 degrees, which is owing to the differences expreſs'd by fig. 2. and not to thoſe of the curvatures or circular places. Yet this variation of 57 ſeconds ariſes only, when the ſpot or coincidence

cidence falls at the extremity of the horizontal ſight or vane, or a whole inch (in an inſtrument of 20 inches radius) from the centre, and then only in the altitudes or arches of about 44 or 90 degrees. And in theſe, at the diſtance of $\frac{1}{2}$ an inch from the centre, the variation is but $\frac{1}{4}$ ſo much, *viz.* about 14″; and at $\frac{1}{4}$ of an inch not 4″; at the centre it is preciſely true. As, therefore, an obſervation may be taken with it in $\frac{1}{4}$ of the time, that *Davis*'s quadrant (in which 3 things muſt be brought to meet) in a general way requires: Conſidering this and the vaſt importance of ſuch diſpatch, in the caſe of great altitudes, or of tempeſtuous ſeas, or beclouded ſkies, it is preſumed the inſtrument thus made will be judged preferable to all others of the kind, hitherto known. Some maſters of veſſels, who ſail from *Philadelphia* to the *Weſt-Indies*, have got of them made as well as they can be done there; and have found ſo great an advantage in the facility and in the ready uſe of them, in thoſe ſoutherly latitudes, that they reject all others. And it can ſcarce be doubted, but when the inſtrument becomes more generally known, it may more univerſally obtain in practice.

In 1730, after *Thomas Godfrey* was ſatisfied in this improvement, he applied himſelf to think of the other, *viz.* the reflecting inſtrument by ſpeculums, for a help in the caſe of longitude, tho' it is alſo uſeful in taking altitudes; and one of theſe, as has been abundantly proved by the maker, and thoſe who had it with them, was taken to ſea, and there us'd in obſerving the latitude the winter of that year. In *May*, 1732 Mr. *Logan* tranſmitted an account of it to Dr. *Halley*. This, on his part, is all the merit he has to claim in either of theſe inſtruments.

Mr. *Logan* only adds, that the bow had beſt be an arch of about 100 degrees, well graduated, and number'd both ways; the radius to be of 20 or 24 inches; the curve at the centre to be $\frac{1}{20}$ of the radius on each ſide, that is $\frac{1}{10}$ of it in the whole; the radius of that curve $1\frac{44}{100}$ parts of the radius of the inſtrument; that the glaſs for the ſolar vane ſhould not be leſs, but rather larger, than a ſilver ſhilling, with its vertex very exactly ſet: And that the utmoſt care be taken to place the middle of the curve at the centre exactly perpendicular to the line or radius of 45 degrees. As the obſerver muſt likewiſe take care, that the 2 vanes on the limb be kept nearly equi-diſtant from that degree; to which he only adds, that it may be beſt to give the horizontal vane only one aperture, and not two. The reſt he ſuppoſes may be left to the workmen.

Note,

Note, that the radius of the quadrant being divided into 20 equal parts, the centre × (Fig. 3.) of the curvature of the horizontal vane, A B, muſt be 12 $\frac{8}{10}$ of thoſe parts from the centre C of the quadrant. The breadth A B or *g h* of that vane ſhould be $\frac{1}{10}$ of the whole radius, that is $\frac{1}{20}$ on each ſide of the centre C.

The Deſcription and Uſe of an Inſtrument for taking the Latitude of a Place at any time of the Day; *by Mr.* Richard Graham. Phil. Tranſ. N° 435. p. 450.

THE neceſſity of finding the latitude a ſhip is in, is too well known to be inſiſted on; frequent opportunities of obſerving the latitude muſt, conſequently, be of very great advantage to navigation. The method uſually practis'd is by taking the ſun or ſtars meridian altitude or zenith diſtance: In this caſe, if the ſun do not ſhine but for ſome ſmall time only, before and after noon, tho' it be clear all the reſt of the day, it is of no uſe for this purpoſe: In 1728 Mr. *Fatio* propos'd a method for finding the latitude, from 2 or more obſervations of the ſun, or ſtars, at any time, the diſtance of the ſaid obſervations in time being given by a watch: But as his method requires a vaſt number of computations, and a great deal of ſkill in ſpherical trigonometry, it has very ſeldom been made uſe of, and never but by good Mathematicians. The inſtrument here deſcribed will anſwer the ſame end, and has the following advantages; *viz.* 1. It may be very eaſily underſtood by ſeamen. 2. It immediately ſhews the latitude of the place. 3. It gives the time of day at ſea, when no other inſtrument can. 4. It may be made as large; and conſequently as accurate, as is deſir'd.

The deſcription of the inſtrument is as follows; A B C (Fig. 4. Plate III.) repreſents part of the hemiſphere of a large globe (half the globe, and the part below the tropic, are cut off, that it may take up the leſs room) A C half the equator, divided into 12 hours above, and 180 degrees below, and ſubdivided into minutes, as is likewiſe the lower tropic D D; E E a moveable graduated meridian, turning on the axis F F; G an index to fix it (by means of the ſcrew H) to any hour; I *i* I a circular beam-compaſs, the centre I *i* to be fixt on the meridian to any degre and minute of declination, by the method commonly call'd *Nonius*'s diviſions; *k* the point for drawing arches, which is likewiſe fixt to any degree and minute by the ſame method. As the meridian is at ſome diſtance from the globe,

globe, L is a piece of braſs to fix on the meridian, marked with *Nonius*'s diviſions, with a point reaching down to the interſection of the arches, by which means the diſtance of the ſaid interſection from the equator, or its latitude, is found. The degrees and minutes may likewiſe be ſhewn by diagonal lines.

The uſe of the inſtrument is, as follows.

Propoſition 1. From 2 obſervations of the height of the ſun, the diſtance of the ſaid obſervations in time, being given by a watch, as alſo the declination of the ſun; to find the latitude of the place and hour of the day.

1. When the ſhip is at reſt, that is, at anchor, or in a calm; ſo as to have little or no progreſſive motion.

Caſe 1. Suppoſe the ſun in the equator, on the day of obſervation: Fix the center of the beam-compaſs at 0 degree, or at the equator; and move the point *k* to the zenith diſtance (the complement of the altitude taken by the uſual inſtruments) and from any hour as from C, deſcribe an arch of a circle with the ſaid point, as *bc* (*Ex.* 1. Fig. 4.) Suppoſe 8 hours after by your watch, you have another obſervation; move the meridian 8 hours farther to *d* and fix it there; and with the zenith diſtance then obſerved, deſcribe another arch as *ef*; the point where it cuts the former is the place of obſervation; and its diſtance taken on the meridian from the equator ſhews its latitude; and the minutes reckon'd on the equator from the meridian to C and *d* (the times of obſervation) ſhew what thoſe hours were.

Caſe 2. When the ſun has declination. Fix the centre of the beam-compaſs on the meridian, to the proper degree of declination for the day of obſervation, and proceed as before.

Caſe 3. If the obſervations are at a greater diſtance than 12 hours, but in the ſame day. Make uſe of the complement to 24 hours of the diſtance in time, and take the declination on the contrary, or lower ſide of the equator; and inſtead of the zenith diſtances, take the nadir diſtances, or altitudes, increaſed by 90 degrees.

Thus, you will find the latitude and time of each obſervation from midnight. In this caſe the beam-compaſs muſt extend to more than 90 degrees.

Caſe 4. If the obſervations are more than a day aſunder; as for inſtance a day and two hours, namely 26 hours. Place the centre of the beam-compaſs two hours farther than it was the day before; but in different declinations, according to the table of declination for the ſeveral days.

Caſe

Caſe 5. When the obſervations are made by a ſtar. The centre of the beam-compaſs muſt be ſet to the declination of the ſtar; then proceed as before. To find the hour in this caſe, the right aſcenſion muſt likewiſe be given.

Scholium. The ſame method may be uſeful at land, when no meridian obſervation offers.

2. When the ſhip is in motion.

Caſe 1. Suppoſe the ſun in the equator; the diſtance between the 2 obſervations 8 hours, as before; and the arch *a a a* (*Ex.* 2. Fi. 4.) deſcribed by the zenith diſtance of the firſt obſervation, from the centre C; and the angle *c a b*, 40 degrees, is the angle between the ſhip's way, and the azimuth of the ſun continued (given by the azimuth compaſs) and that during the 8 hours, the ſhip has made one degree or 60 minutes from *a* to *b*, or from the ſun; then as the radius is to the coſine of *c a b*, 40 degrees; ſo is *a b*, 60 minutes, to *a c*, 46 minutes: Add 46 minutes to the zenith diſtance C *a*; and with *k*, the point of the beam-compaſſes ſet at that diſtance, deſcribe the arch *c b e*; then with the zenith diſtance of the laſt obſervation, whoſe centre is *d*, draw the arch *f f*; the point where it cuts the arch *c b e* is the place where the ſhip was laſt; and its diſtance taken on the meridian from the equator ſhews its latitude; the minutes reckon'd on the equator from the meridian to *d* (the time of the laſt obſervation) ſhew the hour, or its diſtance from 12 o'clock.

Caſe 2. If the ſhip had ſailed from *a* to β, or towards the ſun: The coſine of the angle $\beta a \gamma$, or of the angle between the ſhip's way and the ſun, muſt be ſubſtracted from the zenith diſtance of the firſt obſervation.

N. B. Only the 2 arches *c b e*, *f f*, are to be drawn on the globe, the reſt being added here, to ſhew the reaſon of the conſtruction.

Caſe 3. To find the latitude of the firſt place. From the equator, with a pair of compaſſes, take the diſtance ſail'd 60 minutes; and with one foot in the interſection of the arches *b e*, *f f*, the place found before, put the other in the arch *a a a*, the zenith diſtance of the firſt obſervation; and in this inſtance, on the left hand of the azimuth of the ſun, this is the place ſought; and its diſtance taken on the meridian from the equator, ſhews the latitude; and the minutes reckon'd on the equator from the meridian to C, the time of the firſt obſervation, ſhew the hour.

The interval in time, or the degree between the 2 places, ſhewn by the index G, is the difference of longitude.

N. B.

N. B. Thoſe obſervations are beſt, whoſe arches croſs each other almoſt at right angles.

Prop. 2. The zenith diſtance of 2 ſtars, obſerved at the ſame time, their declination and right aſcenſion being known; to find the latitude of the place of obſervation.

Fix the centre of the beam-compaſs to the declination of either of the ſtars, and with the zenith diſtance of that ſtar deſcribe an arch: Move the meridian as many hours farther, as is the difference of right aſcenſion of the other ſtar; and fix the centre of the beam-compaſs to the declination of it; and with its zenith diſtance croſs the firſt arch: The interſection ſhews the latitude of the place of obſervation; and likewiſe the diſtance of the right aſcenſion of the zenith from that of either of the ſtars; by which means the hour may be known.

If a celeſtial globe be made uſe of, then place the centre of the beam-compaſs over the ſeveral ſtars.

The latitude and hour being given, the variation of the compaſs is eaſily known.

N. B. In order to draw arches on the globe, rub ſome powder'd black lead on a piece of paper; lay the ſide which is blacked next the globe, where you imagine the interſection of the arches will be: Then draw them on the clean ſide with the point of the beam-compaſs; and they will appear on the globe; and if the globe be well varniſh'd, they may be rubbed out with bread, or waſh'd out with water.

As altitudes at ſea are now readily taken with great exactneſs, by the quadrant, invented by Mr. *Hadley*; and as the ſaid altitudes are the principles, on which the operations above deſcribed are founded; the previous uſe of that quadrant cannot but be of the utmoſt importance to thoſe who ſhall have occaſion for this inſtrument.

An Abſtract of Meteorological Diaries, *with Remarks thereon; by Dr.* Derham. Phil. Tranſ. N° 435. p. 458.

THE following meteorological obſervations were made at *Hall* in *Saxony*, by M. *Joach. Langen*, *Ann.* 1729; and in 1728 at *Goſlar* in *Lower Saxony*, by Dr. *Joh. Conrad Trumphins*; at *Wittemberg* in *Saxony*, by Dr. *Weidler*; at *Naples* by Dr. *Cyrillus*; at *Southwick* in *Northamptonſhire*, by Mr. *Lynn*; and in *Sweden*, at *Lunden*, *Bettna*, *Upſal* and *Bygdea*, to which the illuſtrious obſervers have added obſervations from *Swenæker*, in *Weſtro-Gothia* (Lat. 58° 10') by

M. *Vaſſenius*, miniſter in *Waſſenda*; at *Wiſingſœ*, by *Magnus Oxelgren*, Lecturer of the College; at *Riſinge* in *Oſtro-Goth*, by *Sueno Laurelius*, Paſtor and Provoſt there. At *Stockholm* (Lat. 59° 30′) by *Joh. Backman*, Citizen; at *Hudickſwald Helſingorum*, by *Olave Broman*, Paſtor there; at *Hernœſand* and *Angermann*, by *Jac. Renmarck*, Lecturer of the Mathematics; at *Lœfanger* and *Umea* (Lat. 63° 43′) by *Bern. Ask*, ſtudent of Divinity; and at *Torneao* in *Weſtro-Goth* (Lat. 65° 43′) by *Abr. Fougt*, Paſtor there.

A Table of the higheſt, mean, and loweſt, barometrical ſtations, in 1728.

	January.			February.		
	High	Mean	Low	High	Mean	Low
Hall	29.4½	28.7½	28.1⅓	29. 7	28.10	28. 2
Goſlar						
Wittemberg	30.2½	29.5¾	28. 9	30.2⅞	29. 9	29. 3
Naples	29.88	29.50	29.12	29.88	29.71	29.54
Southwick	30.08	29.37	28.67	30.10	29.84	29.58
Lunden	30.20	29.46	28.72	30.22	29.52	28.82
Swenæker	30.36	29.68	29. 0	30.35	29.32	28.29
Riſinge	30.20	29.55	28.90	30.20	29.52	28.85
Bettna	30.80	30.10	29.40	30.80	30.12	29.45
Upſale	30.46	29.85	29.24	30.50	29.86	29.23
Hudickſwald	30.50	29.75	29.01	30.56	29.89	29.22
Hernœſand	30.60	29.95	29.30	30.50	29.79	29.08
Bygdea.	30.30	29.75	29.20	30.40	29.67	28.94

March.

	High	Mean	Low		High	Mean	Low
Hall	29. 2	28.9½	28. 5	Riſinge	29.67	29. 0	28.20
Goſlar	31. 3	30.10	30. 6	Bettna	30.21	29.55	28.90
Wittemberg	29.9⅞	29. 5	29.1⅓	Upſale	30.00	29.40	28.80
Naples	29.88	29.63	29.38	Hudickſwald	30.24	29.52	28.80
Southwick	29.88	29.35	28.83	Hernœſand	30.25	29.47	28.70
Lunden	29.81	29.16	28.51	Bygdea.	30.28	25.39	28.50
Swenæker	29.73	29.05	28.37				

April

	April.			May.		
	High	Mean	Low	High	Mean	Low
Hall	28.11	28. 8	28. 5	29.1⅓	28. 8	28.3½
Goslar	31. 4	30.11	30. 6	31. 5	31.0½	30. 8
Wittemberg	30. 0	29.4½	28. 9	30. 1	29. 8	29 2½
Naples	29.88	29.75	29.63	29.80	29.71	29.63
Southwick	29.94	29.48	29.03	29.96	29.51	29.07
Lunden	29.73	29.22	28.51	30.02	29.57	29.12
Swenæker	29.83	29.05	28.27	30.07	29.26	28.45
Risinge	29.70	29.06	28.43	29.95	29.35	28.75
Bettna	30.22	29.63	29.05	30.50	29.90	29.30
Upsale	30.00	29.49	28.98	30.29	29 73	29.17
Hudickswald	30.10	29.60	29.10	30.38	29.79	29.20
Hernœsand	30. 7	29.50	29.07	30.20	29.72	29.24
Bygdea.	29.80	29.40	29.00	30.10	29.62	29.14

	June.			July.		
	High	Mean	Low	High	Mean	Low
Hall	29.0¾	28. 8	28. 3	28.11	28. 8	28. 5
Goslar	31. 4	31.0½	30. 9	31. 3	30.11	30. 7
Wittemberg	30. 0	29.8½	29. 5	30. ¾	29. 7	29.3½
Naples	29.88	29.75	29.93	29.80	29.71	29.63
Southwick	29.93	29.70	29.27	29.95	29.57	29.20
Lunden	29.83	29.52	29.21	29.73	29 38	29.02
Swenæker	29.93	29.49	29.06	30.10	29.19	28.28
Risinge	29.70	29.35	29.00	29.50	29.15	28.80
Bettna	30.20	29.89	29.58	30.12	29.66	29.20
Upsale	30. 9	29.73	29.38	29.91	29.55	29.20
Hudickswald	30.96	30.09	29.22	30.05	29.61	29.18
Hernœsand	30.10	29.71	29.32	29.97	29.53	29.10
Bygdea.	29.96	29.42	28.88	29.85	29.42	29.00

August.

	High	Mean	Low
Hall	29. 0	28.8½	28. 5
Goslar	31. 5	31. 0	30.9½
Wittemberg	30. ½	29. 8	29.4½
Naples	29.88	29.80	29.72
Southwick	30.04	29.51	28.98
Lunden	29.92	29.37	28.82
Swenæker	29.74	29.19	28.64

AUGUST.

	High	Mean	Low
Risinge	29.57	28.86	28.15
Bettna	30.10	29.56	29.02
Upsale	29.91	29.45	29.00
Hudickswald	29.90	29.48	29.07
Hernœsand			
Bygdea	29.96	29.58	29.20
Torneao	29.85	29.56	29.27

	SEPTEMBER.			OCTOBER.		
	High	Mean	Low	High	Mean	Low
Hall	29. 1	28.10	28. 8			
Goslar	31. 5	31.0½	30. 8	31. 5	31. 0	30. 6
Wittemberg	30. 0	29.7½	29. 3	30. ½	29. 6	29. 2
Naples	29.88	29.54	29.21	29.80	29 71	29.52
Southwick	30.02	29.53	29.04	29.98	29.31	28.64
Lunden	29.91	29.45	29.00	30.12	29.51	28.90
Swenæker	29.93	29.39	28.86	30.26	29.55	28.84
Risinge	29.95	29.32	28.70	30 16	29.43	28.70
Bettna	30.38	20.83	29.28	30.80	30.02	29.25
Upsale	30.17	29.71	29.25	30.49	29.83	29.16
Hudickswald	30.21	29.74	29.27	30.97	29.99	29.01
Hernœsand						
Bygdea	30. 2	29 53	29.05	30.40	29.70	29. 0
Torneao	29.92	29.56	29.20	29.90	29.58	29.25

	NOVEMBER.			DECEMBER.		
	High	Mean	Low	High	Mean	Low
Goslar	31. 6	30. 5	30. 6	31. 8	31. 1	30. 4
Wittemberg	30. ⅓	29. 0	28. 9	30. 2	29. 6	29. 1
Naples	29.96	29.67	29.38	29.80	29.51	29.21
Southwick	29.95	29.45	28.91	30.04	29.42	28.80
Lunden	29.90	29.26	28.62	29.92	29.32	28.73
Swenæker	29.95	29.14	28.34	30.16	29.53	28.91
Risinge	29.80	29.05	28.30	30.05	29.42	28.80
Bettna	30.40	29.70	29.01	30.70	30.11	29.52
Upsale	30.10	29.44	28.79	30.49	29.87	29.25
Hudickswald	30.22	29.47	29.72	30.60	29.95	29.30
Bygdea	30.24	29.42	28.60	30.50	29.80	29.10

A ther-

A thermometrical table of the higheſt, loweſt and mean ſtations at *Naples*, *Southwick*, *Lunden* and *Upſal*, in 1728.

	JANUARY.			FEBRUARY.			MARCH.		
	High	Mean	Low	High	Mean	Low	High	Mean	Low
Naples	49.05	43. 5	38. 5	48. 0	44. 5	35. 0	39. 5	35. 9	32. 3
Southwick	79.	67	55	80	67	54	69	57	45
Lunden	83	68	54	91	73	56	123	98	74
Upſale.	91. 5	80. 9	68. 3	89. 2	77. 9	66. 6	72. 1	63. 0	53. 9

	APRIL.			MAY.			JUNE.		
	High	Mean	Low	High	Mean	Low	High	Mean	Low
Naples	42. 5	33. 2	24. 0	26. 0	18. 5	11. 0	16. 0	9. 7	4. 5
Southwick	71. 0	53	36	56	40	24	50	35	19
Lunden	148	118	85	172	145	118	176	153	130
Upſale	69. 5	52. 1	44. 7	54. 7	45. 0	35. 3	42. 0	33. 6	25. 2

	JULY.			AUGUST.			SEPTEMBER.		
	High	Mean	Low	High	Mean	Low	High	Mean	Low
Naples	13. 5	8. 2	3. 0	16. 0	10. 0	4. 0	26. 5	17. 1	7. 7
Southwick	50	34	17	56	39	23	65	48	32
Lunden	172	152	132	153	133	113	150	122	94
Upſale	37. 2	31. 0	25. 5	40. 2	32. 8	25. 5	58. 4	46. 2	34. 6

	OCTOBER.			NOVEMBER.			DECEMBER.		
	High	Mean	Low	High	Mean	Low	High	Mean	Low
Naples	40. 0	31. 3	22. 5	48. 0	42. 0	34. 0	54. 5	54. 5	44. 7
Southwick	71	56	41	84	64	43	87	72	58
Lunden	119	98	78	109	87	64	83	71	59
Upſale	68. 2	58	47. 7	98. 0	57. 0	56. 1	89. 6	76. 8	64. 0

A Table

A Table of the Rain *at* Southwick *and* Naples *in the Year* 1728.

	Southwick	Naples.		Southwick	Naples.
	Inch Cent.	Inch Measures		Inch Cent.	Inch Measures
Jan.	4. 00	4. $15\frac{1}{2}$	Jul.	3. 21	0. 00
Feb.	0. 99	0. 00	Aug.	0. 96	. 00
Mar.	3. 27	0. 5	Sept.	0. 86	4. 4
Apr.	1. 97	0. 14	Oct.	2. 79	6. $17\frac{1}{2}$
May	1. 44	0. 00	Nov.	1. 52	2. 7
June	2. 82	1. $2\frac{1}{2}$	Dec.	2. 43	6. $8\frac{1}{2}$

Rain in the whole Year.

At Southwick, *is* 26 *Inches, and* 26 *Centesimals.*

At Naples, *is* 19 *Inches, and* 14 *Measures.*

To make the meteorological observations of 1728 as useful as possible, Dr. *Derham* has put as many of them as he could into tables, to be seen and compar'd at an easy view; but he was obliged to omit such of them, where no account is given of the instruments they us'd, or where none at all were made use of, but only verbal descriptions given, which tables would not admit of. But the places mentioned in the tables had Mr. *Hauksbee*'s glasses.

As to the barometrical observations the Dr. only repeats two things he formerly took notice of, and has had frequent confirmation of *An.* 1728; 1. The great conformity of the ascents, descents, and stations of the mercury in the barometer. 2. That the range of the mercury is much greater in the northerly than southerly climates.

As to the thermometrical observations, he has inserted all that were made with the *Royal Society*'s glasses; but such as were made with other thermometers would have been of little or no use, unless they could have been reduced to some known measure, which only two of the curious observers enabled him to do: But he found that matter so perplex'd and difficult, as not to answer the great trouble of it, especially considering that the tables exhibit observations, made in different and distinct parts, *viz. Italy*, *Germany*, *England*, and *Sweden*; by which an estimate, may, in some measure, be made of the temperature of those different climates of the world. In order to which the

Dr.

Dr. takes notice, that the point of extreme heat in the *Royal Society*'s glaſſes is 5 degrees above 0, and of freezing at 65 degrees; and if we caſt our eye upon the ſeveral months, particularly thoſe of winter; eſpecially if we conſider what the Dr. remark'd in 1727 from Dr. *Cyrillus*, concerning the freezing point, which at *Naples* is at 55 degrees, at *London* 65 degrees, and at *Chriſtiana* and *Bengal*, probably as different. Conſidering theſe things it is ſurpriſing, that the heat and cold of thoſe diſtant places is not as different as the northerly and ſoutherly ſituations. But at *Lunden* he was ſurpriſed to find the thermometer much lower in the warmer months than at *Upſal*, or any other of the *Swediſh* places, till he found that in all thoſe months, they had continual cold and rain, when the other places mention little but fair, or cloudy, and but little rain or cold. And this reminds him of a former obſervation, *viz.* that cold is the parent of wet, eſpecially in ſummer.

As to the winds and weather, ſo many are the places of obſervation, and ſo various the obſervations, that it is almoſt impoſſible to give a tolerable abridgment of them, and the Doctor's remarks therefore on the foregoing years, eſpecially on the ſame places and parts of the world, (*vide Phil. Tranſ.* N° 433. *p.* 334, *&ſeq.*) muſt ſuffice here.

The quantity of rain and ſnow was obſerv'd at *Naples*, *Riſinge*, *Bettna*, *Upſal*, *Hudickſwald*, and *Southwick*; but he finds no deſcription of the inſtruments with which the obſervations were made, nor of the meaſures, except at *Southwick* and *Naples*, where the depth is meaſured by *Engliſh* meaſure; the *Southwick* by inches, and hundreth parts of an inch; and the *Naples*, by *Engliſh* inches, and the obſerver's meaſures, 23 of which make an *Engliſh* inch.

Here follows an account of ſuch meteors as the illuſtrious obſervers have taken notice of, together with ſome of the Dr's own that happened about the ſame time.

In the night after *March* 20. a *lumen boreale* was obſerv'd at *Bettna*; and at half an hour after 8 on *March* 22. the Dr. obſerv'd at *Windſor* an unuſual ſort of ſtreaming, in which the columns were not (as uſual) conical or pointed, nor riſing towards the *Zenith* point; but were of parallel ſides, and roſe perpendicularly to the horizon. They were very bright, emitting a light equal to that of the moon in her quarters: They alſo roſe from a bank of vapours, not curv'd at top (as uſual) but lacinated or broken.

Like-

Likewiſe on *March* 24, the curious *Bettna* obſerver ſaith there was obſerved the night before, a *lumen efflammans boreale*, which was alſo ſeen at *Læfanger*.

On *Auguſt* 26. at night, there was a remarkable *lumen boreale*, at *Bettna*: And the night before at 10 hours 20 minutes *p. m.* a gentleman, going from the Doctor's houſe, ſaw towards the eaſt, about 30 degrees high, a ball of fire about 4 inches diameter, blazing and ſtanding ſtill at firſt; and preſently after running towards the north; and in about 5 minutes or more he heard an exploſion like thunder; its blaze emitted a light equal to that of the moon at full.

At the ſame time the new-papers ſay, 'a light in the sky, 'like a comet, was ſeen at *Wattford* in *Hertfordſhire*, with 'ſparks of fire iſſuing from its tail; that then it broke out 'with a prodigious luſtre, like the ſun, which laſted not long, 'and was followed with a terrible clap of thunder, the ſtars 'twinkling all the while, and not a cloud to be ſeen.' Which clap, the Dr. doubts not, was the ſame his friend heard, and which was 5 or more minutes in its paſſage to *Upminſter*.

At *Bettna*, *lumina borealia* were ſeen on the nights after *Sept.* 18, 19, and 24; the 2d of which cover'd half the heavens. And on *Sept.* 21. about 10 hours *p. m.* the Dr. obſerv'd at *Upminſter* an unuſual ſort of tan-coloured thick vapours towards the N. W. *b.* N. but withal lightſome, and ſuch as the ſtars might be ſeen through. And after ſome time they ſent forth in divers places ſtreaming lances, which gently and gradually came and went.

On *October* 13. the Dr. ſaw that uncommon ſort of ſtreaming at *Redbridge*, near *Southampton*, the account of which is publiſhed in *Phil. Tranſ.* N° 410. and the ſame night at *Bettna* there was a *lumen boreale eructans flammam*, as the obſerver expreſſes it: At *Læfanger* alſo thoſe ſtreamings were on the ſame night, and on the 15, 18, 19, and 23d.

On *October* 19. there was a parhelion at *Lunden*, and on the 22d a *lumen boreale* at *Wittemberg*.

On *November* 12. we had conſiderable ſtreaming at *Windſor*; and likewiſe at *Bettna* and *Umea*; and on the 29th at *Læfanger*; and again at *Umea* on *Dec.* 24.

The Dr. having receiv'd the curious obſervations S *Poleni* made at *Padua* for ſix years (vide *Phil. Tranſ.* N° 421. *p.* 201. *& ſeq.*) ſubjoins ſuch of them as are conformable to his own.

A Table

A Table of the Rain at Padua, *in the Years*

	1725	1726	1727	1728	1729	1730
	Inch Dec.	Inch Dec.	Inch Dec.	Inch Dec.	Inch Dec.	Inch Dec.
Janu.	0. 521	1. 355	5 .955	4 .278	1 .085	0 .112
Febr.		1 .460	1 .073	1 .050	1 .245	2 .906
March	0 .889	3 .168	1 .878	4 .832	2 .902	4 .592
April	4 .019	3 .998	0 .498	1 .419	2 .768	1 .638
May	3 .625	1 .368	3 .530	3 .403	2 .634	4 .467
June	0 .036	2 .608	2 .476	2 .103	3 .134	6 .205
July	2 .297	2 .357	2 .930	4 .016	4 .526	2 .339
Auguſt	5 .185	1. 268	5 .067	5 .186	0 .578	4 .269
Sept.	2 .647	2. 900	4 .164	6 .948	3 .267	1 .090
Octob.	7 .104	0 .179	6 .576	5 .163	6 .994	5 .254
Nov.	3 .636	2 .277	5 .091	6 .836	4 .186	0 .534
Dec.	0.030	2 .390	7 .169	7 .599	2 .804	0 .894
Year	29.989	25.328	46.407	52.833	35.423	24.300

From this table M. *Poleni* obſerves, that the *Februaries* were the drieſt months, and 1726 the drieſt year in all the ſix; and that the *Octobers* were the wetteſt months, and 1728 the wetteſt of all the 6 years.

Farther alſo he ſaith, that in the 4 ſeaſons of the years (reckoning their beginning from the 10th day of their reſpective months, *viz. December*, *March*, *June*, and *September*) leſs wet falls in winter and ſpring, than in ſummer and autumn; and that the wet weather increaſes, as the ſeaſons advance; that in winter the wet is leaſt; that it increaſeth in ſpring; is more in ſummer; and moſt of all in autumn.

For proof of this M. *Poleni* hath made a table of the mean quantities of the rain in the 4 ſeaſons of each of the 6 years; the ſums of which 6 years rain are, as follows, in winter 39.490 inches; in ſpring 52.188 inches; in ſummer 58.25 inches; and in autumn 74.558 inches. But in the many years the Dr. obſerv'd the weather at *Upminſter*, he finds it not ſo.

A Table of the higheſt and loweſt Station of the barometer with the winds and weather in the

Year	Month	Day O. S	Barometer Higheſt	Barometer Loweſt	Winds	Weather at the ſame time.
1725	Janu.	19	30.28		W	Fair.
	Dec.	8		28.56	S W 4	Cloudy.
2726	Nov.	28	30. 18		N	Fair.
	Feb.	13		28.92	S W	Cloudy.
1727	Nov.	20	30.24		N W	Fair.
	Octob.	29		28.80	S 2	Cloudy.
1728	Dec.	2	30.20		N	Thin Clouds.
	Dec.	12		29.00	N W	Small Rain.
1729	Dec.	20	30.30		W	Somewh. cloudy
	Nov.	10		2 8.90	N	Rain.
1730	Dec.	20	30.40		N	Fair.
	Feb.	27		28.98	S E	Sunſh. with Cl

From this table it appears, from the higheſt and loweſt ſtations in the 6 years, that the greateſt range of the barometer is 1. 84 inches; but at *Naples* it is only 94 centeſimals of an inch; and what it was in other places the Dr. has given ſome account of in his remarks on 1727.

The illuſtrious obſerver hath alſo been very curious, and ſedulous, in his obſervations of the thermometer. But the Dr. had not ſo much knowledge of his thermometer, as might enable him to compare his obſervations with his own.

S. *Poleni* hath alſo compared with his own, the quantities of rain, and the barometrical range obſerved at *Paris*, by M. *De la Hire*; and he finds that the rain at *Paris* is 16. $4\frac{7}{12}$ lines, and the barometrical range 2 $\frac{5}{12}$ lines more than that at *Padua*.

The laſt thing S. *Poleni* takes notice of is the magnetical declination, which he ſaith is 13 degrees weſt and hath decreaſed in the 6 years $\frac{1}{60}^{o}$; that every day there is a ſmall alteration in declination: So that it doth not continue the ſame a whole day together; that the declination of all needles (eſpecially

cially if touch'd by different magnets) is different a few sexageſms. But theſe niceties the Dr. recommends to the enquiry of the curious; becauſe they diſagree with the obſervations of *Gilbert*, and moſt of the magnetical writers.

Experiments and Obſervations on the light, produced by communicating electrical attraction to animate or inanimate bodies; together with ſome of its moſt ſurpriſing effects; by Mr. Stephen Gray. Phil. Tranſ. N° 436. p. 16.

THE ſeveral new electrical diſcoveries M. *Dufay* made, eſpecially that important luciferous one, mentioned in a former *Tranſaction*, put Mr. *Gray* upon making the following experiments.

In ſpring 1733, as Mr. *Gray* had not any ſilk lines by him ſtrong enough to bear the boy, he cauſed him to ſtand on ſome of the electrick bodies; and as he concluded, he found the effect the ſame, as mentioned by M. *Dufay*. He waves mentioning the particulars of the experiment; but proceeds to thoſe that were ſuggeſted to him upon M. *Dufay*'s ſaying, that theſe ſnappings or ſparks are not excited, if a piece of wood, or any other matter than a living body, be paſſed over the perſon ſuſpended on the lines, unleſs it be a piece of metal: From hence he concluded, that if he ſuſpended the metal upon ſilk lines, or laid it upon any of the electric bodies, the effect muſt be the ſame, when the metal had been made electrical by the tube, and the hand of any one was held near it; and he found it to ſucceed accordingly. He began firſt with ſome common utenſils that were at hand, as the iron poker, tongs and fire-ſhovel; any of theſe being ſuſpended upon lines of the largeſt ſewing ſilk, and then the excited tube being applied firſt to the knob of the poker, and afterwards to the hand, there was the ſnap and pricking felt, as he expected; and the effect was the ſame, when the tube was firſt applied to the other end of the poker. He had by him a three pronged iron inſtrument, which was made many years before; it was deſigned for propping up the obſervatory table, when he obſerv'd the ſpots in the ſun: The prongs were about ½ an inch in diameter, two of them about 22 inches, and the third about 8 inches long, and pointed; this being laid either upon cylinders of glaſs, cakes of roſin, and bees-wax, or on a cake of ſulphur, the tube being applied to the end of any of the legs, while the hand or cheek was applied near the other, both the other legs had the ſame effect, as that to which the tube had been

applied; but by holding his cheek near any of the points of the legs, the pricking or burning pain was much more ſenſibly felt, and was ſometimes felt for ſeveral minutes after. He was not ſo inquiſitive at that time about making the experiment in the dark, that he might ſee the light proceeding from the iron, nor thinking that the electricity communicated to the metals would have produced ſo ſurpriſing phenomena, as by the following account of the experiments will be deſcribed.

As to the ſucceſs in repeating M. *Dufay*'s experiment at Mr. *Wheeler*'s, we procur'd ſilk lines ſtrong enough to bear the weight of his foot-boy, a good ſtout lad: Then having ſuſpended him upon the lines, the tube being applied to his feet or hands, and the finger of any one that ſtood by held near his hands or face, he found himſelf prick'd or burnt, as it were, by a ſpark of fire, according to M. *Dufay*'s account, and the ſnapping noiſe was heard at the ſame time: But it did not ſucceed when we applied our hands to any part of his body through his cloaths, except upon his legs, upon which he felt the pain thro' his ſtockings, tho' they were very thick ones.

Being deſirous to make the experiments upon another ſpecies of animals; we took a large white cock, and ſuſpending him upon the lines firſt alive, the effect was the ſame as on the boy, whether we applied our fingers to any part of his body, or our cheek to his beck, comb or claws; then the cock was killed and put on the lines again, and we found very little, if any difference, from the effect it had on us when the cock was living: We then cauſed the cock to be ſtript of all his feathers, and the difference from what has been before ſaid was not very conſiderable.

We took a large ſirloin of beef, that came from an ox that had been kill'd two days before, and ſuſpended it on the ſilk-lines: Then upon holding the fingers near any part of it, there was a ſnapping, and the fingers were puſh'd or prick'd; but the ſnapping was thought not to be quite ſo loud, as when the experiment was made on the cock.

We cauſed to be made an iron rod, 4 foot long, and about half an inch diameter, pointed at each end, but not ſharp, being left about the bigneſs of a pin's head; this was ſuſpended on the lines; then the tube being rubbed, and held near one end of the rod, and then the finger or cheek, put near either end of the rod, the effect was the ſame, as when an animal had been ſuſpended on the line, with reſpect to the pricking pain we felt.

At

At night we made the luminous part of the experiment, ſuſpending the iron rod on the ſilk lines; then applying one end of the tube to one end of the rod, not only that end had a light upon it; but there proceeded a light at the ſame time from the other, extending in form of a cone, whoſe vertex was at the end of the rod; and we could plainly ſee that it conſiſted of threads, or rays of light, diverging from the point of the rod; and the exterior rays incurvated. This light is attended with a ſmall hiſſing noiſe; every ſtroke we gave the tube cauſes the light to appear: The hiſſing ſeems to begin at that end of the rod next the tube; and as it comes, encreaſes in loudneſs; but it is ſo ſmall as not to be heard without good attention, and by thoſe only that ſtand at the end of the rod from whence the ſaid light proceeds.

Mr. *Godfrey* being deſirous to ſee theſe experiments, Mr. *Gray* repeated them, by laying a rod of iron upon a cake of ſhell-lac, which was laid upon a glaſs-veſſel; and the effects were much the ſame with what has been mentioned above.

After his return to *London*, which was in *September* 1733, Mr. *Gray* cauſed to be made 3 iron rods, one 4 foot long, and two, 3 foot long each; one of theſe was made tapering towards the ends, and pointed as that of 4 foot was; the other pointed at one end, and the other end not pointed, the diameter of the rods about half an inch; they were firſt forged, then fil'd and burniſh'd. With theſe he made the following experiments: When any of them were laid upon the brims of hollow cylinders of glaſs well warm'd, or upon cakes of roſin and bees-wax, or upon thoſe of ſulphur, the phenomenon was the ſame, as when they had been ſuſpended on ſilk lines: But now he diſcover'd a very ſurpriſing one, *viz.* that after the tube had been applied, and the light ſeen at both ends, upon his going to the other end of the rod, when there was no light to be ſeen, upon holding his hand at ſome diſtance from it, then moving his hand towards it with a pretty ſwift motion, there iſſued from that point of the rod a cone of light, as when the tube had been applied to the end; and upon repeating this motion of his hand, the ſame phenomenon appeared for 5 or 6 times ſucceſſively, only the rays were each time ſhorter than the other: theſe lights were attended with a hiſſing noiſe. That light which appears upon that end next the tube, when it is held obliquely to the axis of the rod, has its range tending towards it: All the time he is rubbing the tube, theſe flaſhes of light appear upon every motion of his hand up or down the tube;

tube; but the largeſt flaſhes are produced by the motion of his hand downwards.

When 2 or 3 rods are laid either in a right line, or forming any angle with each other; or either touch, or are at a ſmall diſtance from one another, the tube being applied to one of their ends; the fartheſt end of the farther rod exhibits the ſame phenomena, as one ſingle rod does.

An experiment with the rod pointed at but one of its ends; when the tube is applied to the other end, the point exhibits the ſame appearance and a like effect with the rods, that are pointed at each end; but the great end of the rod, when the hand or cheek is applied near it, gives but one ſingle ſnap; but this is much louder than the greateſt of thoſe from the point of the rod, and one feels a little more pain by it.

He caus'd forge an iron ball, and then turn and burniſh it; it was 2 inches diameter, and placed on a wooden ſtand that had a ſmall concave on the top, in which the ball was placed: The ſtand being ſet upon a cylindric glaſs, then the excited tube being applied near the ball, there proceeded a ſtream of light from it, with a ſmall hiſſing noiſe: Then putting his finger or cheek near the ball, there was no ſnapping, nor any pain felt; yet there appear'd a very bright light.

The rod of 4 foot long, being placed upon a ſtand, that had a croſs arm with a groove in it to receive the rod; and then the ſtand being placed on the glaſs cylinder, they were ſet at ſuch a diſtance as that one of the points of the rod might juſt touch the ball over againſt its centre; then going to the other end of the rod with the excited tube, he applied it as uſual; when he came to the ball, the hand or cheek being near it, caus'd a loud ſnap, compared to thoſe made by the points of the rods, and the pain of pricking or burning was more ſtrongly felt, the light was alſo brighter and more contracted: He then placed the rod with its point at an inch diſtance from the ball, and applying the rod as before, he came to the ball, and touching it with his hand or finger, there not only appear'd a light on the ball, but there alſo proceeded a pencil of light from the point of the rod, after the ſame manner, as when the experiments had been made with the rods only.

The following experiment was made with the 4 foot rod, and a braſs plate 4 foot ſquare: This was placed upon a ſtand; ſo that the plate ſtood perpendicular, the ſtand being ſet on the cylindric glaſs; then the rod with its ſtand and

glaſs

glaſs was ſet in ſuch manner that one point of it was about an inch from the centre of the plate; then the tube being applied to the other end of the rod, and after going to the plate, on ſtriking it gently with his finger on the backſide, a light appear'd upon the plate, and at the ſame time the pencil of light came out from the point of the rod; and when his hand or cheek was held near any of the angles of the plate, there came a light from thence with a ſmall hiſſing noiſe, and the pricking was felt, as when the experiments were made with the pointed rods.

A pewter plate being laid upon the ſtand, which had been ſet upon a glaſs cylinder, applying the tube firſt and then the finger, there appear'd a light upon the plate, and the end of the finger was puſh'd; and when the cheek was held near the edge of the plate, there was a ſnapping heard, but not ſo loud as when the iron rods were us'd. He then fill'd the plate with water, and applying the tube and finger as before, there was the ſame light, puſhing of the finger and ſnapping, as when the experiment was made with the empty plate. When the experiment is made with water by day light, by applying the end of the finger near the ſurface of the water, it appears to riſe in a little hill, and upon the ſnapping noiſe falls down again, putting the water into a waving motion, near the place where the water had riſen.

He then took a wooden diſh, and placed it upon the ſtand, firſt empty; then applying the tube and the finger near the diſh, there appear'd a light, but no puſhing of the finger, nor ſnapping: He then fill'd the diſh with water, and the tube being held over the ſurface of the water, there appear'd a greater light than when the finger had been applied to the empty diſh, but no ſnapping; till by holding the tube after it had been well rubbed, within 2 or 3 inches of the finger that was held near the ſurface of the water; and then the finger was puſh'd, and a ſnapping noiſe heard, as when the experiment was made with the pewter plate.

By theſe experiments we ſee, that an actual flame of fire, together with an exploſion, and an ebullition of cold water, may be produced by communicative electricity; and tho' theſe effects are at preſent but *in minimis*, it is probable, in time there may be found out a way to collect a greater quantity of it; and conſequently to increaſe the force of this electric fire; which, by ſeveral of theſe experiments, ſeems

to

to be of the ſame nature with that of thunder and lightning.

An Account of M. Seignette's Sal polychreſtus Rupellenſis, *and ſome other chemical* Salts; *by M.* Geoffroy. Phil. Tranſ. N° 436. p. 37.

M. *Seignette's ſal Polychreſtus Rupellenſis* is a *tartarum ſolubile*, compos'd of cream or cryſtals of tartar, and the fixed ſalt of the *kali* of *Alicante*, well depurated. This ſalt is very ſingular: For, tho' it be a fixt alcaline ſalt, it has the peculiar property of cryſtallizing, nor does it eaſily diſſolve in the open air, as other fixt ſalts do; but on the contrary calcines therein like vitriols or *Glauber*'s ſalt. Another peculiar property, M. *Geoffroy* obſerv'd of it is, that if it be ſaturated with a vitriolic acid, and the liquor be evaporated, there reſults a ſalt that reſembles *Glauber*'s ſalt, and has all the properties requiſite to make M. *Seignette*'s ſalt. In order to which;

Take of the ſalt of *kali* well depurated 1 *lb*, diſſolve it in water, add thereto about 1 *lb. ſs.* of cryſtals of tartar; boil the whole in order to diſſolve the cryſtals: But the exact proportion of cryſtals of tartar can be determined no more in this operation, than in making the *tartarum ſolubile*; either becauſe the ſalt of *kali* has retain'd more or leſs humidity in its cryſtallization, or becauſe the tartar has more or leſs impurities in it. But if there be too much tartar in the alcaline liquor, after the fermentation is over, filtrate the liquor, and as it cools, the ſuperfluous tartar will fall to the bottom. After the ſeparation of the tartar from the liquor, evaporate the lixivium by a gentle fire, ſet it in a cool place to cryſtallize, and you will have very fine cryſtals. If the liquor be evaporated a little too much, there will be no cryſtals of ſalt formed, but the liquor will be converted into a hard tranſparent maſs, not unlike glue. But if you diſſolve this maſs again, you may make it cryſtallize, as upon diſſolving M. *Seignette*'s ſalt.

This ſalt purges very well, from one to two ounces, diſſolv'd in a quart of water.

M. *Seignette*'s cryſtallized alcaline ſalt, is the ſalt of *kali*, that does not diſſolve in the air. M. *Geoffroy* was actually at work in perfecting this ſalt, in examining that of *kali*, and comparing it with *borax*. From this laſt he extracts *Glauber*'s

ſalt

alt, by mixing it with oil of vitriol. The mixture of borax 4 ounces with one ounce and a drachm of vitriol, upon ſublimation, gives the *ſal ſedativum*, deſcribed by M. *Homberg*; and the reſidue expos'd to a ſtrong fire affords *Glauber*'s ſalt. M. *Geoffroy* found out a method to ſhorten this operation: For, inſtead of ſubliming this ſalt, he gets it by cryſtallization in light foliated *laminæ*. This ſalt, whether ſublimated, or cryſtalliz'd, has the property of diſſolving in ſpirit of wine; and if you ſet this ſpirit of wine on fire, its flame is green. Spirit of wine has no effect on *borax*; the oil of vitriol, digeſted with ſpirit of wine, communicates no greenneſs to its flame; it is therefore requiſite, that the *borax* be united with an acid, in order to produce this green flame.

Sal ſedativum made by cryſtallization cryſtallizes in a peculiar manner: This operation is performed with 4 ounces of *borax*, 1 ounce and 1 drachm of concentrated oil of vitriol, the moſt fixt and weighty that can be had. The *borax* is put into a glaſs retort, the oil of vitriol is pour'd on it, and then half an ounce of common water. This mixture being expos'd to a fire, gradually increaſed, after the phlegm has paſs'd off, and even while it is a paſſing, there riſe flowers, or a volatile ſalt in very beautiful foliated *laminæ*, ſome of which melt by the heat of the fire. After the operation, the fineſt of theſe flowers, which are round the neck of the retort, are gather'd; and thoſe that are grey are thrown upon the remaining maſs; which maſs is diſſolv'd in water, filtrated and evaporated ſlowly. Sometimes even without evaporation, the ſhining talcous *laminæ* are to be ſeen in the liquor. In 24 hours the liquor is pour'd off theſe *laminæ*; they are waſh'd in fair water, ſet to drain, and then to dry in a ſtove.

If theſe cryſtals do not calcine in the ſtove, or in the ſun, it is a ſign there is nothing cryſtallized but the *ſal neutrum*: If they do calcine, it is a ſign that there is ſome *Glauber*'s ſalt mixed: And then this ſalt muſt be diſſolv'd again in hot water, and re-cryſtalliz'd. No body before M. *Geoffroy* thought of extracting this ſalt by cryſtallization, being always ſublimed hitherto.

An Account of a Machine *for changing the Air of the Room of ſick People in a little time, by either drawing out the foul Air, or forcing in freſh; or doing both ſucceſſively, without opening Doors or Windows; by Dr.* Deſaguliers. Phil. Tranſ. N° 437. p. 41.

DECB (Fig. 1. Plate IV.) repreſents a caſe, containing a wheel of 7 foot diameter, and 1 foot thick; being a cylindrical box, divided into 12 cavities by partitions directed from the circumference towards the centre, but wanting 9 inches of reaching the centre; being open towards the centre, and likewiſe towards the circumference; and only clos'd at the circumference by the caſe, in which the wheel turns by means of a handle, fixt to its axis A, which axis turns in 2 iron forks, or half concave cylinders of bell-metal, ſuch as A, fixt to the upright timber or ſtandard A E.

From the middle of the caſe on the other ſide behind A, there comes out a trunk or ſquare pipe, call'd the ſucking pipe; which is continu'd quite to the upper part of the ſick perſons room, whether it be near or far from the place where the machine ſtands, in an upper or lower ſtory, above or below the machine. There is a circular hole in one of the circular planes of the machine, of 18 inches diameter round the axis, juſt where the pipe is inſerted into the caſe, whereby the pipe communicates with all the cavities; and as the wheel is turned ſwiftly round, the air, which comes from the ſick perſon's room, is taken in at the centre of the wheel, and driven to the circumference; ſo as to go out very ſwiftly at the blowing pipe B, fixt to the ſaid circumference.

As the foul air is driven away from the ſick room, the air in the neighbouring apartments will gradually come into the room thro' the ſmalleſt paſſages: But there is a contrivance to apply the pipes which go to the ſick room to the blowing pipe B, while the ſucking pipe receives its air only from the room where the machine ſtands. By this means freſh air may be driven into the ſick room after the foul has been drawn out.

This machine would be of great uſe in all hoſpitals, and priſons: It would alſo ſerve very well to convey warm or cold air into any diſtant room; nay, to perfume it inſenſibly upon occaſion.

Fig. 2. repreſents the inſide of the flat of the wheel, which is fartheſt from the handle, and next to the ſucking pipe;

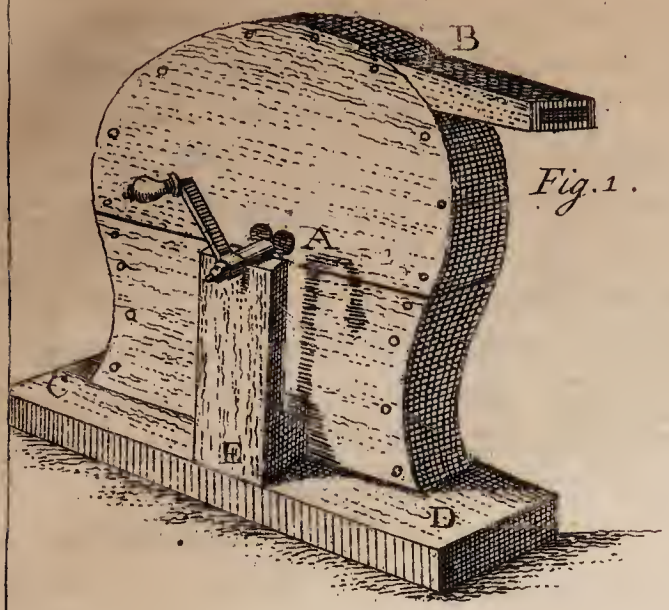
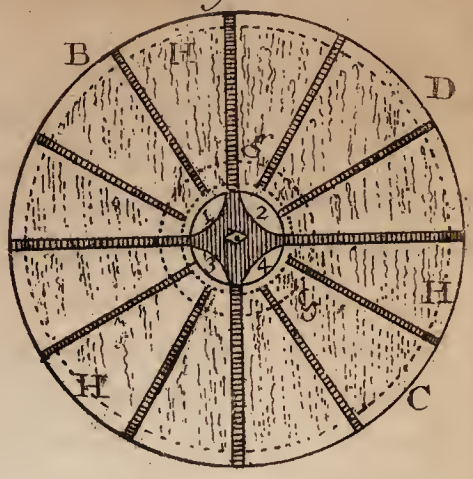


Fig. 1.

Fig. 2.



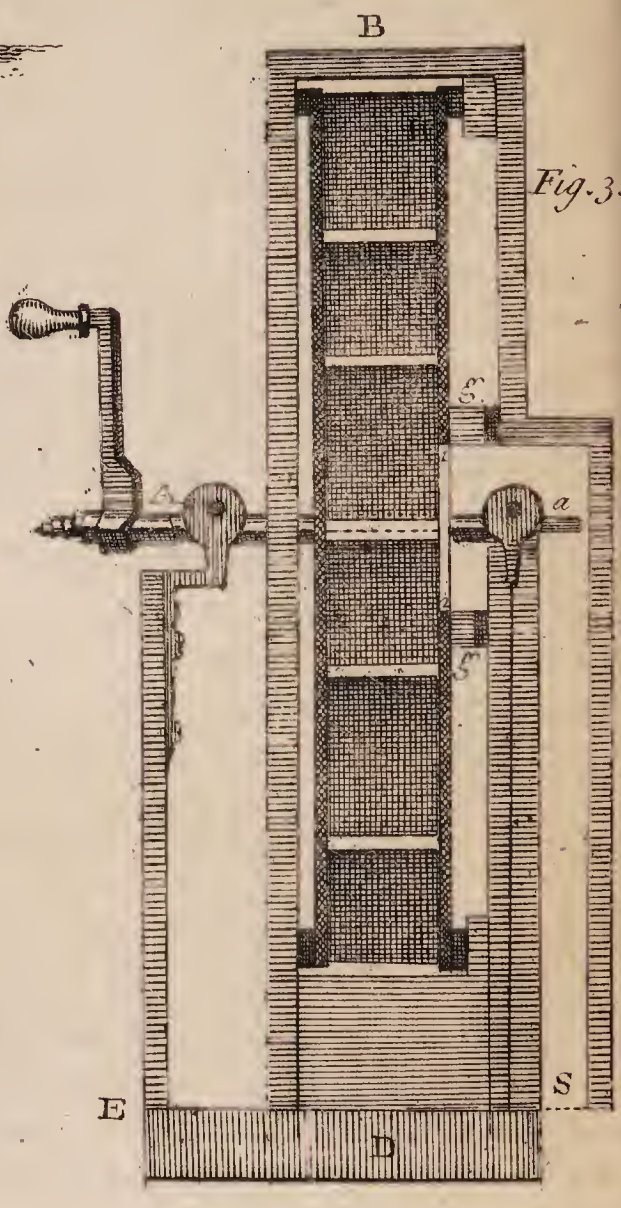
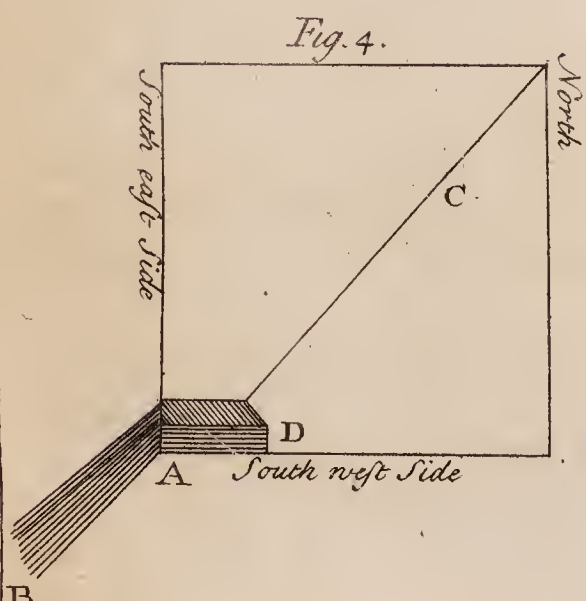


Fig. 3.

Fig. 4.



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1, 2, 3, 4 represents the cavity or hole, which receives the air round the axis, having a circular plate of iron round it to hold all firm; which plate is made fast to the wood and to the iron cross that has the axis in it; *ggg*, a prick'd circle, represents a narrow ring of thick blanketting (which by pressing against the outside case, whilst it is fixt to the outside of the flat of the wheel) makes the passage into the wheel tight; H H H is another circle of blanketting, likewise fixed to the outside of the wheel, and rubbing against the case, that the air violently driven against the inner circumference of the case may have no way out, but at the blowing pipe at B. There is on the outside of the other flat of the wheel, where the handle is fixt, a ring of blanketting, like H H H, opposite to it; but none opposite to *ggg*; because the wood there is not open, but comes home close to the axis.

Fig. 3. represents a vertical section of the wheel and case, a little forward of the axis, drawn by a scale twice as large as that of the other 2 figures: A *a*, the axis supported by the irons A *a*, cylindrically hollow'd, excepting the upper part, where a pin keeps in the axis; B D the case with the sucking pipe S *a*; E A the prop for one end of the axis; 1, 2, the opening into the wheel; *gg* the eminence of the wood, to which is fixed the small ring of blanketting; the four black marks, one of which is near H, represent the sections of the other two rings of blanketting.

A Calculation of the Velocity of the Air, *mov'd by the new invented centrifugal* Bellows *of 7 foot diameter and 1 foot thick within; by Dr.* Desaguliers. Phil. Trans. N° 437. p. 44.

WHEN the wheel revolves upon its axis (every revolution in this machine being performed by a man with very little labour in about half a second of time) the air may be consider'd as divided into as many concentrical circumferences, as there are particles of air contain'd between the least and greatest circle; consequently, the centrifugal forces will be as the *radii*; that is in an arithmetical progression.

Let R = radius of the greatest circle be 3. 5 foot
r = radius of the least circle 0. 75
m = radius of the middle circle 2. 125

$$= r + \frac{R - r}{2} = \frac{R + r}{2}.$$

$v =$ the velocity, or ſpace, deſcribed in the middle circle in a ſecond of time, upon ſuppoſition that the wheel performs 2 revolutions in a ſecond. } 26.21

$S =$ the ſpace deſcribed by the action of gravity in a ſecond of time. } 16.1

$s =$ the ſpace that a particle of air receding from the centre would in a ſecond of time deſcribe by the action of the centrifugal force at the circumference of the middle circle.

$2m : v :: v\ s : s$; therefore, $\frac{vv}{2m} = s$ by *Huygens*'s rule. Let G and c expreſs the force of gravity, and the centrifugal force at the middle circle. Since the ſpaces deſcribed in the ſame time by the action of 2 forces are as thoſe forces, $S : s :: G : c$, and $\frac{sG}{S} = c$, and ſubſtituting in this expreſſion $\frac{vv}{2m}$ inſtead of s, we have $\frac{vvG}{2mS} = c$; and putting $\frac{R+r}{2}$ inſtead of its equal m, $\frac{vvG}{\overline{R+r} \times S} = c$. So that the ratio of gravity to the centrifugal force, at the middle circle, is that of G to $\frac{vvG}{\overline{R+r} \times S}$, or that of 1 to $\frac{vv}{\overline{R+r} \times S}$; which being multiplied by the number of the revolving circles $R - r$ gives for the preſſure of the column of air $R - r$ proceeding from gravity $R - r$, and the preſſure proceeding from the centrifugal forces $\frac{\overline{R-r} \times vv}{\overline{R+r} \times S}$; wherein $R - r$ being a factor common to both may be thrown out of the expreſſion: And ſince the velocities produced from different preſſures are as the ſquare roots of the preſſures, the velocity which gravity would give from the natural weight or preſſure of $R - r$ will be to the velocity which the ſame column would have from the preſſure occaſion'd by the centrifugal force, as $\sqrt{1}$, or 1 to $\sqrt{\frac{vv}{\overline{R+r} \times S}}$.

Laſtly, ſince the velocity proceeding from the action of gravity upon a column $= R - r$ is always a known quantity; it may be call'd $= a$ (in this caſe equal to 15.38 foot *per* ſecond) and conſequently, the velocity proceeding from the centrifugal

trifugal force will be $a \times \sqrt{\frac{vv}{R+r \times S}}$, or $av \times \sqrt{\frac{1}{R+r \times S}}$, or $\frac{av}{\sqrt{R+r \times S}}$: That is, in this machine $\frac{15.38 \times 26.71}{\sqrt{4.25 \times 16.1}} =$ 49.67 foot *per* ſecond. And if we add to this the velocity of the outer circle, in the tangent of which the air eſcapes, which in the ſuppoſition we made of 2 revolutions in a ſecond of time) is 44 foot *per* ſecond, we ſhall have it = 93.67 foot *per* ſecond.

N. B. This calculation ſuppoſes the bore of the ſucking pipe ſufficiently great to furniſh as much air as would eſcape, according to this velocity: But in this machine the ſucking pipe being no greater than the ajutage or blowing pipe, the velocity proceeding from the preſſure occaſion'd by the centrifugal force, and from the velocity in the tangent (which may be repreſented by a column of air of ſufficient height to give the velocity of 93.67 foot, which is 145.882 foot) muſt be divided into 2 equal parts, one half employ'd in ſucking, and the other in blowing; therefore, the half of 145.882 foot, which is 72.941 foot, will repreſent the height of a column of air, that would occaſion the ſame preſſure, with which the centrifugal force and the circular motion act in this machine; and a column of this height producing a velocity of 68.53 foot *per* ſecond. This number will expreſs the velocity with which the air is ſuck'd into the wheel; and the ſame number will likewiſe expreſs the velocity of the air out of the blower, proceeding from the centrifugal force, and the circular velocity of the outer circle, which is the real velocity of the ſtream of air out of the blower of this machine, *viz.* 68.53 foot *per* ſecond; which is at the rate of a mile in about 77 ſeconds, or about 7 miles in 9 minutes.

The Uſes of the foregoing Machine; *by the ſame.* Phil. Tranſ. N° 437. p. 47.

ONE of theſe centrifugal wheels is fixt in a room above the *Houſe of Commons*, to draw away the hot ſteam ariſing from the candles, and the breath of the company in the houſe, when it is very full, in warm weather; as alſo afterwards to drive in a ſtream of freſh air, to ſpread uniformly all over the houſe, by coming in at the middle of the cieling.

Beſides

Beſides the uſes of this machine for ſick rooms and priſons, for warming, cooling, or perfuming any chambers at a diſtance; it may alſo ſerve in a man of war, to take away the foul air between decks, occaſioned by the number of men in the ſhip, and to give them freſh air in a few minutes. In every part of the veſſel every foul hole may be rendred wholeſome, and even the ſtench and foul air from the ſurface of the bulge-water may be carried off. As to mines, the machine muſt prove of excellent uſe. For, as the damps, (either fulminating, which taking fire, deſtroy the men and ruin the works, or arſenical, which kill by their poiſonous nature) are ſome ſpecifically lighter, and ſome ſpecifically heavier than common air; this centrifugal wheel can in a little time drive down air thro' wooden trunks, or launders, of 7 inches bore, in ſuch quantities into the deepeſt mines, as to cauſe all the light damp to come out at the top of the pit; or by only altering 2 ſliders, ſuck away all the heavy poiſonous damp, whilſt wholeſome air goes down from above ground into the pit, ſo as to fill all the ſubterraneous caverns with freſh and wholeſome air.

Likewiſe a great many of the difficulties, which attend the carrying on ſubterraneous paſſages for the conveyance of water from mines (call'd ſoughs, adits or drifts) may be remov'd by help of this wheel: For, the freſh air may be driven in a very little time to the place where the men are at work; tho' at the diſtance of 2, 3, or 4 miles: and therefore alſo to any intermediate ſpace: Whereas the practice now is, either to make a double drift with communications between the two for the circulation of the air, or to ſink perpendicular ſhafts or pits from the top of the hill over the adit; both which methods are very expenſive, and will, (the doctor doubts not) upon trial, be out-done by the application of this machine.

The caſe of a cataleptic Woman; by Mr. Richard Reynell. Phil. Tranſ. N° 437. p. 49.

ONE *Anne Bullard*, a Servant maid, about 21 years of age, had for ſome time been irregular in her menſes, and very much afflicted for the loſs of a friend. *July* 10. 1730. ſhe complained of a pain in her head, ſickneſs in her ſtomach, with a general diſorder; and ſhe took *Gaſcoign*'s powder for a ſweat: Next morning about 9 o'clock, ſhe was found in bed, ſenſeleſs, ſtiff, and void of feeling, with her eyes ſhut. When Mr. *Reynell* came, he found her in a true cataleptic fit, ſenſe-

leſs,

eſs, without motion, her limbs very ſtiff, but warm, and not eaſily bent: But in whatever poſture any limb was put, it continued in the ſame, whether erect or reclin'd: Her reſpiration was good; but her pulſe low, and irregular: She had no catchings nor convulſive motions; but could, by no means, be brought to herſelf. A vein was opened in the arm, and 12 ounces of blood taken away: ſhe bled freely, and came a little to herſelf, but could not ſpeak. He then gave her the following draught. ℞ *Aq. Menth. Rutæ, Bryon. Co. aa* ℥vj. *Sal. volat. Corn. C.* ℈ſs. *Sacchar. albiſſ.* ℈ij *f. hauſt.* and 5 ſpoonfulls at pleaſure of the following Julep. ℞ *Aq. Puleq. Rutæ, Menth. aa* ℥ij. *Aq. Brion. Co. Nephrit. aa* ℥iſs *Tinct. Caſtor.* ℥ij. *Sacchar. albiſſ. q. ſ. f. Julapium.* In a few hours ſhe came to herſelf; and upon asking whether ſhe knew how ſhe was taken? She replied, that ſhe had been reſtleſs and uneaſy till about 4 o'clock in the morning, when ſhe believ'd ſhe fell into the diſorder ſhe was found in; but remember'd nothing that had happened beſides. She complained of a dizzineſs in her head, with a violent pain in the fore-part of it, ſickneſs at her ſtomach, and ſhe was a little feveriſh. He gave her the following vomit at 4 o'clock in the afternoon. ℞. *Aq. Cardui benedict.* ℥j. *Pulv. ipecacuanh.* ℥ſs. *Vitriol. alb. depurat. gr.* vi. *oxym. ſcillit.* ℥jſs. *f. hauſt.* The vomit work'd kindly, and ſhe ſeem'd reliev'd by it. About 6 o'clock in the evening another fit returned, much in the ſame manner as before; but ſhe ſoon came out of it; and then ſhe took the draught with the volatile ſalt of hartſhorn, as before; and he applied a large bliſter to her back, and two more to her arms: About 9 the ſame evening ſhe had a ſtrong convulſion fit, with catchings, grinding of the teeth, and a great tremor, neither of which ſhe had before: She had a ſtool the preceding night, but none that day. He gave her the following draught at night going to reſt. ℞. *Tinct. Hier. cum vino fact.* ℥jjſs. *Aq. Ment.* ℥vj. *Sp. Lavend. Co.* ℥ſs. *f. hauſt.* and ſhe continued taking the draught, with the volatile ſalt, *&c.* every 4 hours. *July* 12, ſhe had been light-headed all night, with little or no reſt; the bliſters were dreſſed, which diſcharg'd plentifully, and the tincture had given her three ſtools in the night, which had made her a little faint; her pulſe was low, and her water pale. He ſaw her in the evening, when ſhe had ſlept pretty well, after which ſhe was refreſh'd; the pain in her head but little, her ſtomach eaſy, and he found her in every reſpect better. The draughts were continued every 6 hours,

and

and when faint or ill, she took of the above mentioned julep. *July* 13, in the morning he found her head easy, her water higher coloured: She was allowed broth and food of easy digestion, which agreed very well with her: She sat up in the afternoon, but was faint, and her head giddy; but when in bed she was better: She had no stool that day. He gave her a draught with the volatile salt, &c. at night going to rest, and the following purge next morning. ℞. *Tinct. Hier. cum vino fact.* ℥j ʃs *Syr. è Spinâ cerv. Aq. puleg. aa* ℥vj *Sp. Lavend. Co.* ℥j *f. haust. cum regimine cap. July* 14, the purge worked 5 times; she eat a little dinner, and was easy: But upon walking about the room, her head was giddy, and she trembled very much; but when in bed she was better. He gave her the following draught at night going to rest. ℞. *Aq. Rutæ, puleg. Bryon. Co. aa* ℥vj *Sp. Corn. C. opt. gutt.* 40. *Tinct. Castor.* ℥j *Sacchar. albiss. paululum f. haust. July* 15. she complained when up of a numbness in her legs, and a pricking in them, like what happens when the legs are (as we commonly call it) asleep: Her appetite was better, and she was in every respect mended. She took the following medicines. ℞. *Pulv. rad. Valerian. sylv.* ℈ij *P. Castor. Russ.* ℈j *Asæfetid.* ℥j *Tinct. Castor. q. s. f. Massa pilular. cujus formentur pilul.* N° 40. of which she took 4 twice every day with a small draught of the following julep. ℞. *Aq. Cerasi. nigr.* ℥vj *Aq. Rutæ, Pæon. Co. aa* ℥ij *Sp. Lavend. Co.* ℥vj *Syrup. Caryoph. q. s. f. Julap.* of which she likewise took 5 spoonfuls at pleasure. The blisters were kept running as long as possible; and when they were dried up, *July* 19, he gave her the same purge as before. *July* 22. she had continued very well, without any return of a fit; but upon cutting an issue in her arm, she fell into a third fit, in which she continued near two hours, but then came to herself and was well that evening. *July* 29. the purge was repeated. *August* 6. she complained of a pain in her head, sickness in her stomach, and some days before she had a shew of the menses, and had vomited near a pint of blood, and was costive: He then advised her to take 2 spoonfulls of *tinctura sacra*, every night, or every other night, going to bed, as she found it necessary, and 40 of the following drops. ℞. *Sp. C. C. opt.* ℥iij *Tinct. Helleb. nigr.* ℥v to be taken twice a day in cammomile tea. She took these medicines about 3 weeks, which answered expectation, and she recover'd.

Sen-

Sennertus in Med. pract. lib. 1. *c.* 30 says, 'that a catalepsis is a case so rare, that it is supposed hardly one physician in a hundred has seen a cataleptic patient; therefore the history of a catalepsis, when it happens, is carefully to be noted.'

An Account of the Operation of the Fistula Lachrymalis; by Dr. Francis Joseph Hunauld. Phil. Trans. N° 437. p. 54.

DR. *Hunauld* remarks, that the intention in the operation of the *fistula lachrymalis* in destroying the *os unguis* and *saccus lachrymalis*, thro' which the tears naturally distil into the nose, is to procure them a new passage thither, by the hole thus artificially made: Wherefore in order to keep the sides of this hole asunder, to prevent its filling up, and to render the flesh, which forms its circumference, hard, and callous, as it were, a tent made of prepared sponge, &c. is put into this new passage, and is continued therein a month or two. Notwithstanding this precaution, it happens but too often, that the tears, instead of keeping the road prepared for them with such care, flow over the lower eye-lid, as before the operation, and occasion a weeping, which now becomes past remedy.

It is easy to prove, that those very means, which are used after the operation to make the tears distil into the nose, are generally the cause of the subsequent weeping: For by filling the wound with small plegets, and putting a tent into the hole that was made, the orifice of the little common canal, that serves to convey the tears into the *ductus lachrymalis*, suffers a compressure, and is rendred hard, thick and callous; whereby as its diameter is very small, it is easily stopped up. The contusion made on this little orifice, and round about it, brings on a suppuration; after which the parts coalesce, and the orifice of this small canal closes up. The *pus* or *sanies*, that in the course of the distemper flowed back, both thro' the common canal, and the small canals, which are a continuation of the *puncta lachrymalia*, has sometimes occasion'd excoriations; in consequence of which there happens a regeneration of flesh during the dressings, a small matter of which is sufficient to stop up such slender ducts. In fine, those small canals, thro' which nothing passes for a month or two, that the dressings last, either close by their proper springiness, or their diameters are lessened by their small vessels becoming varicose. It is certain, that injections are sometimes made thro' the *puncta lachrymalia*; but the propelling force of these injections overcomes those resistances, which the cause, that naturally drives the tears

into the *puncta lachrymalia*, is not in a condition to get the better of

Thus it appears from the detail of the accidents enumerated, and which generally happen, more or leſs, that while the artiſt is endeavouring to preſerve a clear paſſage for the tears into the noſe, he labours, without deſigning it, to ſtop the entry of the upper part of their canal. The beſt way to avoid part of theſe accidents, and keep open the new canal from the eye to the noſe, is preciſely to do nothing. This is what experience has confirm'd, and what theory, well underſtood, will likewiſe give a clear conception of.

It is a thing not very eaſy to determine, how the tears, and the liquid that is continually found on the ſurface of the eye, in order to preſerve the cleanneſs and tranſparency of the *cornea*, can paſs thro' the *puncta lachrymalia*. It is moreover obſerv'd, that when one lies in bed, this liquid enters into thoſe *puncta lachrymalia*, which in that poſition are higher than the eye, as well as into the *puncta lachrymalia* of the oppoſite eye. The aſcent of liquors in capillary tubes above the level, might be propoſed to explain this laſt fact. One might likewiſe in ſome circumſtances imagine that the road which the tears keep, to paſs from the eye into the noſe, to be a ſiphon, whoſe ſhort leg is divided into two. Theſe two ſimple ideas, however, are not ſufficient to account for the phenomenon under conſideration. The following *rationale* ſeems to the Dr. quite as ſimple, and more accurate.

The air preſent at the orifice of all the ducts, which have any communication with the *trachæa*, is by its proper weight determined to enter them, when the reſiſtance happens to be diminiſhed. Thus as, during inſpiration, it paſſes thro' the mouth and noſtrils, ſo it likewiſe enters the *puncta lachrymalia*: and muſt neceſſarily carry along with it, towards the *puncta lachrymalia* and their ſmall canals, the moiſture that lubricates the ſurface of the ball of the eye, as it mixes with it: It is therefore already eaſy to perceive, that in order to preſerve to the tears their new and artificial road into the noſe, one need only commit the whole care to the continual paſſage of the air and tears. It is well known in ſurgery, that it is very difficult, not to ſay impoſſible, to effect a re-union in a part that ſerves as an emunctory to a liquor conſtantly flowing to it.

Now let us examine, if nature alone can ſtop the hole made by the operation. It will not be imagined, that from the remains of a bony *lamina*, ſo thin as the *os unguis*, a ſufficient quantity

tity of offifying juice can work out to ſtop it up. The *perioſteum* and *ſaccus lachrymalis* are too much lacerated, to think it poſſible for them to repair of themſelves what they had loſt. Nor will it be believ'd, that the *membrana pituitaria* can eaſily fill up the hole made in it. Theſe are the parts concerned in the operation: But even if they be granted to be more diſpoſed to a re-production than they really are, ſtill the air and tears will always be able to preſerve themſelves a paſſage into the noſe.

Wherefore, after having deſtroy'd the *ſaccus lachrymalis* and *os unguis*, inſtead of introducing an extraneous body capable of making the orifice of the ſmall common canal into the *Ductus lachrymalis* become callous, and of drawing on a ſuppuration, the communication between the noſe and the eye muſt be left entirely diſengaged; and liberty by this means be given to reſpiration, to make both the air alone, and the air mixt with the tears, to paſs continually through it.

In fine, the action of theſe fluids may be aſſiſted by the application of collyrium's, and by making frequent injections into the *puncta lachrymalia*; which, beſides the common effects that may be naturally expected from them, will contribute to prevent the juice, that re-unites the wound made in the skin, from over ſtreightning the canal.

The Cauſe of the general Trade-winds; by Mr. Hadley. Phil. Tranſ. N° 437. p. 58.

MR. *Hadley* thinks the cauſes of the general trade-winds not to have been fully explained by any of thoſe who wrote upon that ſubject, for want of more particularly and diſtinctly conſidering the ſhare the diurnal motion of the earth hath in the production of them: For, tho' this has been mentioned by ſome amongſt the cauſes of thoſe winds, yet they have not proceeded to ſhew how it contributes to their production; or elſe they have applied it to the explication of theſe phenomena, upon ſuch principles as will appear upon examination not to be ſufficient.

That the action of the ſun is the original cauſe of theſe winds, all are agreed; and that it does it by cauſing a greater rarefaction of the air in thoſe parts, upon which its rays falling perpendicularly, or nearly ſo, produce a greater degree of heat there than in other places; by which means the air becoming ſpecifically lighter than the reſt round about, the cooler air will by its greater denſity and gravity, remove it out of its

place to ſucceed into it itſelf, and make it riſe upwards. But it ſeems this rarefaction will have no other effect than to cauſe the air to ruſh in from all parts into the part where it is moſt rarefied, eſpecially from the north and ſouth, where the air is cooleſt, and not more from the eaſt than the weſt, as is commonly ſuppos'd: So that ſetting aſide the diurnal motion of the earth, the tendency of the air would be from every ſide towards that part, where the ſun's action is moſt intenſe at the time; and ſo a N. W. wind be produced in the morning, and a N. E. in the afternoon alternately, on this ſide of the parallel of the ſun's declination, and a S. W. and S. E. on the other.

That the perpetual motion of the air towards the weſt cannot be deriv'd merely from the action of the ſun upon it, appears more evidently from this: If the earth be ſuppos'd at reſt, that motion of the air will be communicated to the ſuperficial parts, and by little and little produce a revolution of the whole the ſame way, except there be the ſame quantity of motion given the air in a contrary direction in other parts at the ſame time, which is hard to ſuppoſe. But if the globe of the earth had before a revolution towards the eaſt, this by the ſame means muſt be continually retarded: And if this motion of the air be ſuppos'd to ariſe from any action of its parts on one another, the conſequence will be the ſame. For this reaſon it ſeems neceſſary to ſhew, how theſe phenomena of the trade-winds may be caus'd, without the production of any real general motion of the air weſtwards. This will readily be done by taking in the conſideration of the diurnal motion of the earth: For, let us ſuppoſe the air in every part to keep an equal pace with the earth in its diurnal motion; in which caſe there will be no relative motion of the ſurface of the earth and air; and conſequently, no wind: Then by the action of the ſun on the parts about the equator, and the rarefaction of the air proceeding therefrom, let the air be drawn down thither from the N. and S. parts. The parallels are each of them bigger than the other, as they approach to the equator, and the equator is bigger than the tropics, nearly in the proportion of 1000 to 917; and conſequently, their difference in circuit about 2083 miles, and the ſurface of the earth at the equator moves ſo much faſter than the ſurface of the earth with its air at the tropics. From which it follows, that the air as it moves from the tropics towards the equator, having a leſs velocity than the parts

of

of the earth it arrives at, will have a relative motion contrary to that of the diurnal motion of the earth in thoſe parts, which being combined with the motion towards the equator, a N. E. wind will be produced on this ſide of the equator, and a S. E. on the other. Theſe, as the air comes nearer to the equator, will become ſtronger, and more and more eaſterly, and be due eaſt at the equator itſelf, according to experience, by reaſon of the concourſe of both currents from the N. and S. where its velocity will be at the rate of 2083 miles in the ſpace of one revolution of the earth or natural day, and above 1 mile and $\frac{1}{3}$ in a minute of time; which is greater than the velocity of the wind is ſuppos'd to be in the greateſt ſtorm, which, according to Dr. *Derham*'s obſervations, is not above 1 mile in a minute. But it is to be conſider'd, that before the air from the tropics can arrive at the equator, it muſt have gain'd ſome motion eaſtward from the ſurface of the earth or ſea, whereby its relative motion will be diminiſh'd, and in ſeveral ſucceſſive circulations, may be ſuppos'd to be reduced to the ſtrength it is found to be of.

Thus Mr. *Hadley* thinks the N. E. winds on this ſide of the equator, and the S. E. on the other ſide, are fully accounted for. The ſame principle as neceſſarily extends to the production of the weſt trade-winds without the tropics; the air rarefied by the heat of the ſun about the equatorial parts being remov'd to make room for the air from the cooler parts, muſt riſe upwards from the earth; and as it is a fluid, it will then ſpread itſelf abroad over the other air; and ſo its motion in the upper regions muſt be to the N. and S. from the equator: Being got up at a diſtance from the ſurface of the earth, it will ſoon loſe great part of its heat, and thereby acquire denſity and gravity ſufficient to make it approach its ſurface again, which may be ſuppos'd to be by that time it is arriv'd at thoſe parts beyond the tropics, where the weſterly winds are found. Being ſuppos'd at firſt to have the velocity of the ſurface of the earth at the equator, it will have a greater velocity than the parts it now arrives at; and thereby become a weſterly wind, with ſtrength proportionable to the difference of velocity, which in ſeveral revolutions will be reduced to a certain degree, as is ſaid before of the eaſterly winds at the equator. And thus the air will continue to circulate, and gain and loſe velocity by turns from the ſurface of the earth or ſea, as it approaches to, or recedes from the equator. From what has been ſaid, it follows,

1. That

1. That without the assistance of the diurnal motion of the earth, navigation, especially easterly and westerly, would be very tedious, and to make the whole circuit of the earth would perhaps be impracticable.

2. That the N. E. and S. E. within the tropics must be compensated by as much N. W. and S. W. in other parts; and generally all winds from any one quarter must be compensated by a contrary wind some where or other; otherwise some change must be produced in the motion of the earth round its axis.

An Account of the several Earthquakes, *which have happen'd in* New England, *since the first Settlement of the* English *in that Country, especially of the last* Earthquake Oct. 29. 1727; *by Mr.* Dudley. Phil. Trans. N° 437. p. 63.

THAT *New England* is subject to earthquakes is certain; and we have had frequent accounts of it, since the first settlement of the *English* there, which is about 100 years preceeding 1727. The first and most considerable Mr. *Dudley* finds in history, and which seems to have been much like that on *Oct.* 29, 1727 (*vide* likewise an account of this last in *Phil. Trans.* N° 409. p. 124.) was on the 2d of *June*, 1638. This is said (by the author, a Gentlemen of character and probity) 'to have been a great and fearful 'earthquake: It was heard, before it came, with a rumbling 'noise or low murmur like unto remote thunder; it came 'from the northward, and pass'd southward; as the noise 'approach'd near, the earth began to quake; and it came at 'length with that violence, as caus'd platters, tyles, &c. to 'fall down; yea, people were afraid of their houses; the 'shock was so violent and great, as that some being without 'doors could not stand, but were fain to catch hold of posts, '&c. About half an hour after, or less, came another noise 'and shaking, but not so loud nor strong as the former: 'Ships and vessels in the harbour were shaken, &c.' In 1658, there was another very great earthquake, but no particulars related. *Jan.* 31. 1660 there was a great earthquake. *Jan.* 26, 1662, about 6 o'clock at night there happened an earthquake which shook the houses, caus'd the inhabitants to run out into the streets, and the tops of several chimneys fell down. About the middle of the same night there was another shake; also in the morning following the earth shook again.

again: In 1665, 1668 and 1669, the earth was ſhaken; ſince which we have alſo had ſeveral tremors of the earth, but not very conſiderable, till the terrible earthquake *Oct.* 29, 1727, which terrified and amazed the inhabitants from one end of the country to the other. As to the weather that preceeded it: Our winter in *January* and *February* was very moderate, and excepting a few cold days, the weather was pleaſant, and no great froſt in the ground. In the beginning of *March* there was a great deal of ſnow, and ſome cold weather, which ſoon went over; and on the 11th 15 minutes after 4 o'clock, the ſun was eclips'd about 5 digits, as near as Mr. *Dudley* could determine without an inſtrument; after which to the end of the month there was pleaſant weather, rain at times, and once thunder and lightning. *April* for the moſt part had fair pleaſant ſpring weather, and a plentiful rain in the beginning and latter end of the month. The beginning of *May* was alſo pleaſant weather; the 9th, 10th, and 13th, a great deal of rain; the 18th a white-froſt; 24th and 25th cold weather; from that to the end of the month very dry. The beginning of *June* the ſame; abundance of thunder and lightning at times during the whole month. In *July* alſo, tho' we had ſome ſhowers in different places, yet in general it was a very dry ſeaſon, and a great deal of thunder and lightning alſo this month; the 3 laſt days of it ſo exceſſive hot, that there was no working or travelling by day, or ſleeping by night. The beginning of *Auguſt* was alſo exceeding hot, and in particular the firſt day at night from the evening to midnight we had a continued coruſcation or lightning all round the horizon; the like ſcarce ever remembred; it was really terrible; tho' the thunder was not ſevere. Dry weather continued to the 10th, and then we had a plentiful rain all over the province, but our hot weather held on to the end of the month; and till about the middle of *September* we had very hot weather: So that take it all together, Mr. *Dudley* never knew ſo much hot weather in any one ſummer in his time. On the 16. *Sept.* we had ſuch a violent ſtorm from the north-eaſt, as was never remember'd, for the fierceneſs and ſtrength of the wind; it blew down houſes, barns, and a vaſt number of trees in the orchards and woods; there alſo fell a great deal of rain at the ſame time. In the month of *October*, preceding the earthquake, there was a pretty deal of cold weather; on the 23d a great deal of rain with a ſoutherly wind; on the 25th

at

at night, a hard froſt; on the 26th winteriſh weather, and a little ſnow; the 28th cold, the wind at north-weſt; the 29th the wind at north-weſt, tho' little of it, but cold: in the evening quite calm and a clear ſky.

By this ſhort journal of the weather the learned will be enabled, in ſome meaſure, to determine, how far our earth might be diſpos'd to, or prepared for the earthquake that follow'd; firſt, by a long continu'd drought and exceſſive heat, whereby the earth became more porous, and abounded with exhalations or inflamed vapours, and which afterwards being ſhut up by the ſucceeding great rains and froſt, and thereby hinder'd from an ordinary and eaſy paſſage thro' the pores and common vents of the earth, work'd ſo much the more terribly and forcibly on one another. But philoſophers not being hitherto agreed on the nature or certain cauſes of earthquakes, Mr. *Dudley* comes next to enquire what ſort of earthquake that on *Oct.* 29. was.

Gilbertus Jacchæus in his *Inſtitut. phyſicæ, cap. terræ motûs* diſtinguiſhes earthquakes into 4 ſpecies; wherein he agrees with *Ariſtotle* and *Pliny*, according to whom the firſt ſpecies is a ſhake or trembling, and by them liken'd to the ſhaking fit of an ague. Mr. *Dudley* could not hitherto hear of any breach or opening of the earth, thro' the whole extent of this earthquake. It was ſaid by ſome that were abroad, that the earth ſenſibly roſe up, and ſo ſunk down again; but he much queſtions the truth of it: For, had there been any ſuch ſuccuſſion to raiſe the earth to any conſiderable height, the houſes would certainly have tumbled down, or the exhalation have forced its way by ſome breach. Nor was our motion of the earth what *Ariſtotle* and *Pliny* call a pulſe or an intermitting knocking, but one continued ſhake or trembling; and therefore, muſt be ranked under the firſt ſpecies, *viz.* a tremor or ſhake, without altering the poſition of the earth, and left every thing in the ſame ſtate in which it found them, except the falling down of the tops of ſome chimneys, ſtone walls, *&c.* without doors; diſhes and ſome other things within doors; as ſhall be taken notice of farther on.

That this earthquake was of the firſt ſpecies is alſo prov'd from the ſound that accompanied it; ſince tremulous and vibrating motions are proper to produce ſounds, which brings Mr. *Dudley* to the third particular, *viz.* the noiſe or ſound that accompanied, or immediately preceeded this earthquake. This, indeed, was very terrible and amazing; tho' he is apt

to

o think it was thought more conſiderable by thoſe within doors, than ſuch as were without in the open air. Some took this noiſe for thunder; others compar'd it to the rattling of coaches and carts upon pavements, or frozen ground. One of Mr. *Dudley*'s neighbours liken'd it to the ſhooting out of a load of ſtones from a cart under his window. For Mr. *Dudley*'s own part, being perfectly awake, tho' abed, he thought at firſt his ſervants, who lodged in a garret over his chamber, were haling along a trundle-bed: But, in truth, the noiſe that accompanies an earthquake ſeems to be *ſonus ſui generis*, and there is no deſcribing it. This noiſe, as amazing as it was, in an inſtant of time, as one may ſay, was ſucceeded by a ſhake much more terrible. His houſe, which is large and well built, ſeem'd to be ſqueez'd or preſs'd up together, as tho' 100 ſcrews had been at work to throw it down; and ſhook not only every thing in the houſe, particularly the bed under him, but the building itſelf, and every part of it ſo violently for the time, that he was really in great fear it would have tumbled down, and his family periſh'd in the ruin. But thro' the good providence of God they receiv'd no harm.

As to the degree or greatneſs of the ſhake; this will be beſt known from its effects. Mr. *Dudley* has already mention'd the falling of the tops of chimneys, diſhes from ſhelves, *China*-ware, *&c.* doors unlatch'd, bells jangling, beds trembling, chairs moving, *&c.* A country farmer told him, he had 40 or 50 rods of ſtone wall thrown down by it: And tho' theſe effects be not very conſiderable, yet he cannot but be of opinion, that this earthquake for its ſpecies was as violent and terrible, as any we meet with in hiſtory: One of Mr. *Dudley*'s neighbours, that was walking home at the very inſtant, told him, the noiſe firſt brought him to a ſtand, and that during the ſhake, the earth trembled in ſuch a manner under him, that he was ſo far from attempting to continue his walk, that it was as much as he could do to keep upon his legs, and he expected every moment the earth would have open'd under him. Another that was riding home ſays, that upon the noiſe the earthquake made, his horſe ſtood ſtock ſtill, and during the ſhake, trembled to that degree, that he thought he would have fallen under him. The houſe dogs were likewiſe ſenſibly affected with the earthquake; ſome of them barking, others howling, and making ſtrange and unuſual noiſe. Nor was the earth alone affected with

this ſhake, but the ſea alſo in the harbours, and the ſhipping ſmall and great much mov'd with it. Mr. *Dudley* does not ſuppoſe it ever happens that earthquakes of this kind, of any extent, are equal or alike in all places; and accordingly he finds by information from the ſeveral towns, that the ſhake was much more moderate in ſome parts of the country than others.

As to the time and duration of the ſhock: The *Boſton* news-papers fix the time at about 40 minutes after 10 o'clock at night; Mr. *Dudley*'s watch was not ſo much by 5 minutes. The 1. of *November* at midnight, which was 3 days after the earthquake, the moon changed. As to the duration of the firſt and great ſhock, whatever others may publiſh, he can by no means ſuppoſe it exceeded the ſpace of a minute, if ſo long; and after this in the ſame night there were 4 or 5 leſſer tremors; and at ſundry times ſince, the earth trembled in different places, even to the 13. of *November*, but without any conſiderable effects or extent.

Laſtly, as to the courſe and extent of the earthquake: *Boſton*, the metropolis of *New England*, lies in Lat. 42 deg. 25 min. N. and 4 hours 43 min. to the weſtward of *London*, as the longitude was ſettled by Mr. *Brattle* of *New England* and Mr. *Hodgſon* of *London*; and making *Boſton* a centre, we have a certain account that this earthquake was felt in *Kennebeck* river to the eaſtward, and at *Philadelphia* to the weſtward, 150 leagues diſtant from each other upon a W. S. W. and E. N. E. courſe nearly; and no part of the intermediate country, that Mr. *Dudley* can underſtand, eſcaped the ſhake: The colonies of *Rhode-Iſland*, *Connecticut*, and *New-York*, that lie between *New England* and *Penſylvania* being all affected, tho' not equally, particularly at *Philadelphia*, a ſmall ſhock. As to the oppoſite line or latitude, as we may call it, of the earthquake; two noted iſlands to the ſouth eaſt, called *Nantucket* and *Martha's Vineyard*, about 90 miles diſtant from *Boſton* (the firſt lying about 12 leagues into the ſea, diſtant from the main land) had the earthquake. The *Engliſh* ſettlements towards the north-weſt, which hitherto did not exceed 40 or 50 miles from *Boſton*, had all of them this earthquake very ſenſibly; and how far it might reach beyond them towards *Canada*, could not hitherto be ſaid. By this calculation Mr. *Dudley* believes it will be found, that this earthquake was of a much greater extent than any hitherto taken notice of in hiſtory: As to the courſe

of

of the earthquake, or where it firſt began, he could not hitherto determine by all the information he could get: For, they write from *Rhode-Iſland*, *Connecticut*, *New-York*, and *Philadelphia*, all to the weſtward, that it was between the hours of 10 and 11 at night. The ſame is likewiſe affirmed from *Piſcataqua*, *Caſco-bay*, and *Kennebeck* river, which are to the eaſtward: So that hitherto it ſeems to him, that the earth, thro' the whole extent aforeſaid, was ſhaken very near at the ſame time. Some of his neighbours are poſitive that it came from the ſouthward; while others again are poſitive, that where they were, it came from the north. But this is not to be wonder'd at; ſince as Mr. *Dudley* ſuppoſes, the ſubterraneous channels or caverns, thro' which the exhalation paſſes, are not in any one continu'd ſtreight line, but branched out, and running upon all points of the compaſs, eſpecially in ſuch a vaſt extent of land.

A neighbour of Mr. *Dudley*'s that had a well 36 foot deep, about 3 days before the earthquake, was ſurpris'd to find his water, that us'd to be very ſweet and limpid, ſtink to that degree that they could make no uſe of it, nor ſcarce bear the houſe when it was brought in; and imagining that ſome carrion was got into the well, he ſearch'd the bottom, but found it clear and good, tho' the colour of the water was turned wheyiſh or pale. In about 7 days after the earthquake, the water began to mend, and in 3 days more it returned to its former ſweetneſs and colour. Mr. *Dudley* was alſo credibly informed, that ſeveral ſprings and good watering places were ſome of them lower'd, and others quite ſunk and loſt with the earthquake. A Clergyman in a town about 20 miles diſtant from *Boſton* aſſured him, that immediately after the earthquake, there was ſuch a ſtink or ſtrong ſmell of ſulphur, that the family could ſcarce bear to be in the houſe for a conſiderable time that night. The like is alſo confirm'd from other places. Perſons of credit do alſo affirm, that juſt before or in the time of the earthquake, they perceiv'd flaſhes of light. A Gentleman of probity from *Newbury*, a town, ſituated between 30 and 40 miles to the N. N. E. of *Boſton*, writes, that at 40 rods diſtance from his houſe, there was a fiſſure of the earth, and near 20 cart-loads of fine ſand thrown out where the ground broke, and water boil'd out like a ſpring, and mixing with the ſand, made a kind of quagmire; but at the time of writing his letter, which was the 21. of *November*, the ſpring was become dry, and the

ground clos'd up again. Since the receipt of this letter, Mr. *Dudley* underſtood, that the ground where this ſand is thrown up, and round about it for a conſiderable diſtance, is a ſolid clay for 20 or 30 foot deep, and nothing like ſand ever to be found there before: So that the exhalation forced this great quantity of ſand thro' a very deep *ſtratum* of clay. He is alſo very well ſatisfied, that the earthquake was more violent in the towns to the north and north-eaſt of *Boſton*, than in thoſe to the ſouthward and weſtward; and in ſome of them that are rocky, the earth ſhook but a few days after.

An Extraordinary Effect of Lightning *in communicating Magnetiſm; by Dr.* Cookſon. Phil. Tranſ. N° 437. p. 74.

A Tradeſman of *Wakefield* in *Yorkſhire*, having put up a great number of knives and forks in a large box; ſome in caſes or ſheaths, and others not, of different ſizes, and different perſons making, in order to be ſent beyond ſea; and having placed the box in the corner of a large room, there happen'd a ſudden ſtorm of thunder, lightning, *&c.* by which the corner of the room was damaged, the box ſplit, and a good many knives and forks melted, the ſheaths untouch'd. The owner emptying the box upon a counter where ſome nails lay, thoſe who took up the knives that lay upon the nails, obſerv'd that the knives took up the nails. Upon this all of them were tried, and found to do the ſame, nay, to ſuch a degree as to take up large nails, packing needles, and other iron things of conſiderable weight. Needles, or other things, placed upon a pewter diſh, would follow the knife or fork, tho' held under the diſh, and would move along, as the knife or fork was mov'd, with ſeveral other odd appearances, one of which is, that tho' you heat the knives red hot, yet their power is ſtill the ſame when cold.

Q. How the knives and forks came by this magnetic virtue, or how lightning ſhould be capable of communicating ſuch a virtue?

A farther Account of the extraordinary Effects of the ſame Lightning *at* Wakefield; *by the ſame.* Phil. Tranſ. N° 437. p. 75.

THIS ſtorm of thunder and lightning happen'd the latter end of *July*, 1731; and not only broke the glaſs and iron frames of the croſs chamber windows, but at the ſame time

ſplit

ſplit ſome ſtudds in the corner of a wood-houſe; and paſſing into a room, likewiſe ſplit a large deal box, which ſtood in the ſouth corner of the room, where the lightning enter'd, and diſpers'd a great many dozen of knives and forks, which were put up in the box, all over the room.

Some of the knives and forks were melted, others ſnapped in ſunder; others had their hafts burnt; others their ſheaths either ſinged or burnt, and others not. But what was moſt remarkable, was, that upon laying them on a compter, where there were iron nails, rings, *&c.* it was obſerv'd, that when any of them were taken up, there hung a nail or ring at the end of each of them: Moſt of them were tried, and found to do the ſame.

One of the knives tho' made uſe of for almoſt a year and a half to all manner of purpoſes, yet ſtill retain'd the magnetic virtue to an extraordinary degree.

Fig. 4. Plate IV. repreſents an horizontal plan of the room, box, and knives, and direction of the lightning; A the ſouth angle of the room; B the direction of the lightning; C the polar or magnetic line; D the box with the knives, lying in a direction parallel to the longer ſides of the box.

Q. Whether the knives and forks lying in ſuch a direction as either to coincide, or make but an acute angle with the magnetic line, might any ways contribute to their imbibing this magnetic virtue; ſince a bar of iron, placed in ſuch a direction, ſhall in a little time receive a tranſient polarity; and if it continue a long time in that poſition, a fix'd and permanent one?

Q. Whether the knives and forks lying in ſuch a poſition, and being violently heated by the lightning, might not, as they cool'd, ſtrongly imbibe this magnetic virtue: Since a bar of iron heated and placed in a certain direction to cool, will ſooner imbibe this power than in the ſame direction cold?

Q. The polarity of the compaſs has been alter'd by lightning, as may be ſeen in the *Philoſophical Tranſactions*: Now how ſhould lightning be capable of communicating ſuch a power in this caſe; ſince it is plain that it has taken it away in another.

The Deſcription and Uſe of an arithmetical Machine, invented by M. Chriſtian Ludovicus Gerſten. Phil. Tranſ. N° 438. p. 79.

SIR *Samuel Morland*, for ought M. *Gerſtern* knows, was the firſt who undertook to perform arithmetical operations by wheel-work. To this end he invented two machines; one for addition and ſubſtraction; the other for multiplication, which he publiſhed in *London* in 1673, in ſmall twelves. He gives no more than the external figure of the machines, and ſhews the method of working them. But as by this every one, who has any skill in mechanics, will be able to gueſs, how the internal parts ought to be contriv'd; ſo it cannot be denied, that theſe are two different machines, independent of each other; that the laſt, which is for multiplication, is nothing other than an application of the *Neipperian* bones on flat moveable diſks; conſequently, that his invention alone is not fit to perform juſtly all arithmetical operations.

After Sir *Samuel*, the celebrated M. *Leibnitz*, S. *Poleni*, and M. *Leupold* took this undertaking in hand, and attempted to perform it after different methods.

The firſt publiſhed his ſcheme in 1709, in the *Miſcellanea Berolinenſia*; but then he only gave the outward figure of the machine. S. *Poleni* communicated his, explaining at the ſame time its internal conſtruction, in his *Miſcellanea* of the ſame year 1709. M. *Leupold*'s machine, together with thoſe of M. *Leibnitz*, and S. *Poleni* were inſerted in his *Theatrum arithmetico-geometricum*, publiſhed at *Leipſick* in 1727, after the author's death, yet imperfect, as is owned in the book itſelf.

Beſides theſe, M. *Gerſten* learned from ſeveral *French* journals, that *Charles Paſcal* invented one, which however he never had the ſight of. The deſcription of this machine is ſince publiſh'd by M. *Gallon* in his collection of machines and inventions, approved by the academy of ſciences at *Paris*, (publiſh'd in *French* at *Paris*, *An.* 1735. in *quarto* in ſix tomes) in tome IV. p. 137; and likewiſe another by M. *Leſpine* tom. IV. p. 131. and three more by M. *Hellerin de Boiſtiſſandeau*, tom. V. p. 103, 117, and 121.

M. *Gerſten* took the hint of his machine from that of M. *Leibnitz*, which put him upon thinking how the internal ſtructure might be contrived: But as it was not poſſible for him to hit upon the original ideas of that great man, an exact enquiry into the nature of arithmetical operations furniſhed him

him at last with others, which he expres'd in a rough model of wood, and shew'd to some patrons and friends, who encouraged him to have another made of brass. But the want of an artificer, able enough to execute his ideas, made him delay it till 1725, when he set about it, and finished the whole work, fitted to a reckoning not exceeding 7 places. And in *December* of the same year he laid this machine before the Landgrave of *Hesse-Darmstadt*, and the hereditary prince his son, to whom he demonstrated the mechanism of the whole invention.

He owns that the gracious reception it met with from both their highnesses, as well as the express recommendation of M. *de Wieger*, one of the prince's privy council, would have been powerful inducements for him to have published an account of his machine; but for the uncertainty he was under, whether possibly M. *Leibnitz*'s machine had not been brought to its perfection; in which case there is no doubt but the operation of his machine, if it would really perform what is promised in the description, would have been easier than M. *Gersten*'s; and consequently preferable to it, provided its structure did not prove too intricate, nor the working of it take up too much time.

But now being certain, that none of M. *Leibnitz*'s invention has yet appear'd in such a state of perfection, as to have answer'd the effect proposed, and that these of M. *Gersten* differ from all those mentioned above, fancying at the same time, that such as understand mechanics, will find it plain, practicable, and exact, in regard to its various effects, he makes no scruple to publish this invention, the product it is true of his younger years, with the several amendments he has since added to it: The particulars of which are as follows:

There are as many sets of wheels and moveable rulers as there are places in the numbers to be calculated. Fig. 1. Plate V. represents three of them, by which one may easily conceive the rest; A A the first system or the figures of units, according to its internal structure; B B and C C the 2d and 3d system, *viz.* of tens and hundreds *a a a*, according to their external form. First, as to A A, where is a flat bottom of a brass plate, which may be screw'd on, either upon a particular iron frame, or only upon a strong piece of wallnut-tree, doubled with the grain cross'd. In this system are two moveable rules *gggg* and *kkk*; the first of which he calls the

operator

operator, the 2d the determinator. There are beſides, two wheel-works, the upper one is for addition and ſubſtraction; the lower one ſerves for multiplication and diviſion. The upper one is provided firſt with *a*, an oblique ratchet-wheel of 10 teeth, of what diameter you pleaſe; on which, however, depends the length and breadth of the ſyſtem itſelf. This wheel has a ſtop *r*, with a depreſſing ſpring *t*; under the wheel *a* is a ſmaller wheel *b* of the ſame ſhape. Both *a* and *b* are rivetted together, and fixed on a common axis. Under the wheel *b* lies a 3d *f*, which is a common tooth-wheel of 20 or more teeth, according as one pleaſes. It is larger than *b*, and ſmaller than *a*, turns about the ſame axis with the other two above it, and upon it is fixed a ſtop *c*, with the ſpring *d*, which catches the oblique teeth of the wheel *b*. Immediately under this wheel lies the upperpart of the operator, which may be beſt made of iron or ſteel. The wheels may be all made of braſs, except the upper one. The operator is of the ſame thickneſs all over, and in its upper part are fixed as many round ſteel pins, as there are teeth in the wheel *f*, which are to catch the teeth of this wheel, and move it backwards and forwards. The height of thoſe pins ought exactly to anſwer the thickneſs of the wheel *f*. The axis of the wheels *a* and *b* is kept perpendicularly by the bridge *e e*, which is ſcrew'd to the bottom, as appears by the Fig. The operator *g g g g* moves on the ſide, above, and in the middle in two braſs grooves *i i i* and q q; about D it jets out, on which projection a piece of iron *b* muſt be well faſtened, having a ſtrong pin, on which the handle *z* fits, as may be ſeen in the ſyſtem B B. The ſide itſelf D ſlides in another groove *s s*, and in its inner corner joins to it the determinator *k k k*, of the ſame thickneſs with the operator, the ſhape of which is ſufficiently expreſs'd in the Fig. This alſo ſlides up and down, on the one ſide in the groove *s s*, and *v* on the other ſide, where it is ſmalleſt, in a ſmall piece 1, and where it is broadeſt above in the operator itſelf, which is either hollow'd out into another groove, or fil'd off obliquely. The ſliding part of the determinator ought afterwards alſo to be fitted to it. Its chief part is the lock *u*, ſtanding perpendicular on its broad part: It is drawn ſeparately in Fig. 4. as at B B, on which is the ſliding ſtop *c*, that is preſſed down by its ſpring *d*, but rais'd by the tricker *a a*; that tricker has a pin *b*, on which is ſcrew'd on the ſmall handle *l l* (in the ſyſtems B B and C C, Fig. 1.) In the braſs bottom A A (or *α α* Fig. 1.) you muſt file out 10 ratchet-teeth or kerfs, purpoſely for the ſtop

of

f this lock, or which is better, you may inſert into the braſs bottom a ſmall piece of iron, filed out according to this figure. The partition and length of theſe ratchet-teeth in the bottom muſt fit exactly with the circumference of the wheel *f*, (in the ſyſtem A A, Fig. 1.) with this direction, that if the lock be kept by the uppermoſt tooth in the bottom, the operator cannot be mov'd at all; but when by preſſing down the tricker *a* (Fig. 4.) the determinator is ſhov'd down, and is ſtopp'd by the 2d or 3d tooth in the bottom, the operator being likewiſe drawn down, as far as the determinator permits, makes the ſtop *c* (in the ſyſtem A A, Fig. 1.) ſlide over one or two teeth of the ſecond wheel *b*; conſequently the ſame ſtop *c* muſt ſlide over 9 teeth, when the lock of the determinator will ſtand before the tenth tooth in the bottom, and the operator is pull'd down ſo far. If you have a mind to apply theſe ratchet-teeth on the out-ſide of the plate 2°, 2°, that covers the whole, you may fit the lock to it accordingly: But in this caſe the covering plate muſt be well faſtened.

For multiplication and diviſion, there is properly in each ſyſtem but one wheel likewiſe divided into ten ratchet-teeth, on which is rivetted the round plate *l*, on which are engrav'd the numbers or figures. Theſe wheels have no occaſion for any bridge, but may turn about a ſtrong pin of ſteel, ſolder'd to the bottom. The ratchet-wheel *m m* reſts on one ſide upon the determinator, and upon a piece of braſs of the ſame thickneſs, to which are faſtened the ſtop *n*, and the ſpring *p*. Upon the operator is another ſtop *o*, with its ſpring; which ſtop has a ſmall arm at *o*, which is check'd by a ſmall ſtudd, to hinder the ſpring's preſſing the ſtop lower down than it ought: By which contrivance it is ſo order'd, that after the operator is ſlid down as far as it can go, in being ſlid up again, the ſtop *o* will turn but one tooth of the wheel *m m*. The round plate *l* has in its middle a ſmall hollow axis, on which are turned firſt two ſhoulders, and then a ſcrew: This ſcrew in the ſyſtem A A is an ordinary one, winding from the left to the right.

But as each ſyſtem ought to have communication with the preceeding one, tho' not with that which follows; to this end a projecting tooth of communication made of ſteel 3° is rivetted to the upper plane of the uppermoſt wheel *a*. This tooth muſt be placed exactly on the point of a tooth of the wheel, and by its revolution catches and turns every time but one tooth of the uppermoſt wheel of the preceeding ſyſtem, ſliding over the following one, (if there be any) without

touching it. For this reaſon the planes of the braſs bottoms in all the ſyſtems ought to incline a little. This will beſt appear from the vertical ſection, Fig. 2. (cut in Fig. 1. in the direction from 5 to *f*) in which *a* is the braſs bottom, H H the wood-bottom, *g* the operator, *i* the groove, *f* the 3d common tooth-wheel, *b* the 2d wheel, *a* the firſt or uppermoſt ratchet-wheel, *e* the bridge, 2° the covering plate, and 3° the tooth of communication. All theſe pieces are repreſented of the ſame thickneſs; but every artiſt will eaſily know, where to add or take off.

Fig. 5. ſhews the plan and true diſpoſition of the teeth in the ſeveral uppermoſt wheels; that is to ſay, the parallel lines A B and C D ought always to cut the braſs bottoms (which are like one another in length and breadth) length-wiſe into two equal parts: Then the perpendicular interſection E F will determine the centres *a* and *b* of the two wheels H and G. The ſtop *r* ought every time to hold its wheel in ſuch a manner, that the points of two teeth coincide with the line A B or C D. The obliquity of the teeth is the ſame in both, with this difference, however, that in G, which is a wheel of the ſyſtem A A (Fig. 1.) they are cut in from the left to the right, but in H, (a wheel of the ſyſtem B B) from the right to the left. For, making the work more durable, the teeth are not to be cut out into quite ſharp points, but blunted a little, as in the wheel H. The nicety of the whole machine chiefly conſiſts in placing the centre *a* and *b*, or (which amounts to the ſame thing) after having choſen the breadth of the braſs bottoms in determining the diameter of the uppermoſt wheel: For, if that ſhould prove ſo large, as that the two wheels H and G ſhould very near touch each other; the tooth of communication will be ſhort, its operation will be of a ſmall force, and the wheels themſelves will require a very great exactneſs, leſt by turning about the wheel H, and the tooth of communication ſtanding in the poſition, as repreſented in Fig. 5. a tooth of the wheel H may touch it, and ſtop the motion. Whereas, on the other hand, ſuppoſing the centres at the ſame diſtance, and the diameters of both wheels leſs, the tooth of communication will be longer: Then ſuch an exactneſs is not required in the wheel; yet more force is neceſſary for making the tooth of communication lay hold the better. Furthermore, it will be well to make the undermoſt common tooth-wheel as large as you can.

From the conſtruction of this firſt ſyſtem, with which the 3d, 5th, 7th, &c. entirely agree, one may eaſily imagine the

2d, 4th, 6th, 8th, &c. for every thing there is likewiſe the ſame, only that it is inverted: So that what in the firſt ſtands on the right hand, is in the ſecond on the left hand.

The plate for multiplication has on its hollow axis, as has been ſaid before, two ſhoulders; the lowermoſt of which is very ſmall, the ſum of its height, the thickneſs of the plate of the wheel *m m*, and of the operator, muſt amount to as much as anſwers to the height of the bridge *e e*. On both ends of the braſs-bottoms the two pieces of braſs 4°, 4°, of the ſame height, are rivetted on. This being done, at laſt the covering plates 2°, 2° is prepared and ſcrew'd on the piece of braſs 4°, 4°. If the machine be made pretty large, the covering plate muſt be ſcrew'd faſt, not only to the bridge *e e*, but likewiſe not far from the wheel of multiplication. It muſt be provided not only with round holes, thro' which are to go the axis of each uppermoſt wheel *a*, and the hollow axis of the plate *l*; but it muſt alſo have a long ſlit, in which the operator and determinator may be mov'd up and down; and laſt of all a ſmall window over the plate of multiplication, through which the figure or number engraved on the plate may appear diſtinctly. To the projecting ſcrew *l* of the plate *l*, is fitted an handle *f f*, joined to an index in the ſhape of a ſcythe. The ſcrew in the ſyſtem A A is a common ſcrew; conſequently the roundneſs of the ſcythe muſt turn from the left to the right: But in the ſyſtem B B, where it ought to be inverted, like all the other parts, the ſcythe muſt turn from the right to the left, as in the Fig. The uſe of this is to ſhew which way the wheels are to be turned; and the ſcrews are to prevent the machine's being hurt by unskillful hands.

On the ſide of the determinator, *viz.* on that piece which cannot be preſs'd down, is alſo ſcrew'd a ſmall index, which may be directed to ſuch numbers or figures, as is required. Theſe figures are to be engrav'd in the covering plate, according to the Fig. and the diſtance depends on their ratchet-teeth *e e* (Fig. 4.) in the braſs bottom.

On the axis of each uppermoſt wheel *a* (which axis muſt be made ſquare, as far as it projects over the covering plate) is fixed a thin round ſilver plate *x x* (in the ſyſtems B B and C C) or *a d* in Fig. 3. yet ſo as it may not rub againſt the covering plate. It has a hollow axis *b c* (Fig. 3.) on which is a right or left ſcrew, according to the ſyſtem it belongs to, and a ſmall ſhoulder *c*. To the ſcrew is ſcrew'd the handle *f s* (in the ſyſtems B B and C C Fig. 1.) which is vertically flat

on the extremity, in order to turn by it the plate and wheels. The plate, as appears by the Fig. is divided by three concentric circles into two rings, in the outermoſt of which are engraved the numbers for addition; in the innermoſt thoſe for ſubſtraction. This plate only ſhall hereafter be called the ſilver plate; the firſt ring the addition-ring: the ſecond ring the ſubſtraction-ring: Moreover two indexes *w* and *y* are ſcrew'd to the covering plate; *w* ſhews the numbers of the outermoſt or addition-ring, and *y* thoſe of the ſubſtraction-ring. They have hinges, that they may be lifted up, and the ſilver plate taken out or put in again: Their curvature ſerves for a direction, which way the plates ought to be turned. A skilful artificer will be able to give them a neater and handſomer ſhape than in the draught, where the numbers are not cover'd.

All this being done, there remains now the figures or numbers to be engrav'd, in the following manner: Place each uppermoſt wheel *a* (in the ſyſtem A A) in ſuch a manner, that the tooth of communication be ready to catch (as in G Fig. 5.) which may eaſily be felt. Obſerve in the ſilver plate, where the index *w* points; and there engrave the number or figure 9, lower down in the ſubſtraction-ring, where the index *y* points, engrave the cypher 0. After this divide both rings into 10 equal parts, one of which is already deſign'd for 9 in the addition, and another for 0 in the ſubſtraction ring: Then obſerve which way the wheel turns, if from the right to the left, as in ſyſtem B B: Then you muſt from the engraved number 9 in the addition-ring, towards the right, engrave 0 next; then 1, 2, 3, 4, *&c.* and in the ſubſtraction-ring alſo towards the right, from the already engrav'd 0, firſt engrave 9, then 8, 7, 6, *&c.* in an inverted order. But if the wheel turns from the left to the right, as in the ſyſtems A A and C C, you engrave the numbers or figures in the ſame order, but from the right to the left, as may be ſeen in B B and C C. Fig. 1.

In the multiplication wheels *m m* you muſt conduct the index *f f* exactly to the window, as it is drawn in the ſyſtem B B; mark the place on the round multiplication-plate under the window, and engrave upon it the cypher or 0; then make by two concentric circles a ring upon this plate, and divide this ring into 10 equal parts, and after the 0 (already engrav'd) engrave on the numbers, 1, 2, 3, 4, 5, 6, 7, 8, 9, in the ſame order as it was done in the addition-ring of the ſilver plate of the ſame ſyſtem. Laſt of all, if you think fit, you may ſcrew on thin ivory plates, to note upon them the numbers that

are

are to be calculated; particularly a long ſmall one on that ſide of the ſlit of the determinator, where there are no numbers, and likewiſe two ſhorter broader ones, one under the window of multiplication, the other above the ſilver plate. All this together compoſes a machine, by the help of which you may perform all the four arithmetical rules or operations. The way of working it is, as follows:

1. As to addition: For inſtance, if you are to add 32 and 59; becauſe the hindermoſt ſyſtem A A, in the Fig. which ought to repreſent the place of units, is not cover'd, let us take the ſyſtem B B for the place of units, and the ſyſtem C C for the place of tens; turn the ſilver plates *x x* in theſe two ſyſtems, that the indexes *w w* point to the two numbers 5 and 9; then make the determinators, *l l*, *l l*, point alſo to 3 and 2: Next take one of the two operators, for inſtance, in B B, and pull it down as far as you can, and move it upwards again. This done, the number 1 in the ſilver plate in B B will come by this means under the index *w*, and the number 6 of the ſilver plate in the ſyſtem C C under its index at the ſame time, which is 61, the ſum 59 and 2. After this move the operator of the ſyſtem C C alſo up and down, when inſtead of 6, 9 will come under the index; conſequently, you have 91 under the index's *w w*, which is the ſum required of 59 and 32 added together. The reaſon of it is plain: For, by pulling down the operator of the ſyſtem B B ſo far, the ſtop *c* of the lowermoſt or common tooth-wheel *f* (*vide* ſyſtem A A) will ſlide over two teeth of the middlemoſt ratchet-wheel *b*; and by moving the operator up again, the ſame ſtop *c* will turn the two ratchet-wheels *a* and *b* together, and cauſe the ſtop *r* of the great or uppermoſt wheel *a* to ſlide over two teeth; At the ſame time the tooth of communication 3° will move forward one tooth of the uppermoſt ratchet-wheel in the ſyſtem C C; conſequently, on the ſilver plate in B B, inſtead of 9, the number 1; and in ſyſtem C C, inſtead of 5, the number 6 muſt appear under their indexes *w w*: And ſo for the ſame reaſon having pulled up and down the operator of the ſyſtem C C, the number 6 pointed to by the index muſt be at laſt changed into 9.

2. As to ſubſtraction. Suppoſe 40 the ſum, from which you are to ſubſtract 24: Here you muſt put your ſum 40 in the ſubſtraction-rings; that is to ſay, turn the cypher 0 in the ſyſtem B B, and the number 4 in the ſyſtem C C, under the indexes *y y*, as is repreſented in the Fig. Set the determinators at 24,

as in addition; likewiſe move the operators only once up and down, the remainder 16 will appear under the indexes *y y*. As to the reaſon of this operation; when you conſider, that the numbers in the ſubſtraction-rings are engrav'd in an inverted order, as is ſaid before, you will find that it is the ſame as in addition.

3. As to multiplication: For inſtance; if you are to multiply 43 by 3, bring the 0 in all your addition-rings to the indexes, as alſo in all your multiplication-plates in the windows. Write down (which is more particularly neceſſary if the numbers be larger than in this inſtance) the multiplicand 43 upon the ivory plates near the 2 determinators in the 2 ſyſtems B B and C C: But you may write the multiplicator 3 only on the ivory-plate under the window of the ſyſtem B B: Set the determinators at 43; then move your operators ſucceſſively as often up and down, till there appear in both windows the number 3, then you will ſee on your addition-rings, under the indexes, the product 129.

It is eaſy to underſtand, that as multiplication is nothing elſe than a repeated addition, the machine does alſo perform its operation by a repeated addition only: For, the number 3 which appears in the window of the ſyſtem B B, ſhews how many times you have added the number 3, pointed by the determinator to itſelf, which, when done 3 times, is 9. And ſo the ſame number 3, which appears in the window of the ſyſtem C C after your operation, ſhews how many times you have added the number 4 to itſelf. It is needleſs to obſerve, that beſides the 2 ſyſtems B B and C C, there muſt be ſuppos'd another, not expreſs'd in the Fig. which will ſhew the number 1 of the product 129.

4. As to diviſion. If you are, for inſtance, to divide 40 by 3; ſet your dividend 40 in the ſubſtraction-rings under the indexes *y y*, in the ſyſtem B B and C C; turn the indexes *ff*, *ff* near the windows to make 0 appear; write your diviſor near the determinator of the ſyſtem C C, and ſet the determinator at 3; pull the operator up and down, then you will have 1 under the index *y*, and 1 likewiſe in the window. By this you ſee, that you cannot work farther in this ſyſtem C C; becauſe you cannot ſubſtract 3 from 1: You muſt, therefore, go on, to the other figure of the dividend, *viz.* 0, and in the ſyſtem B B ſet the determinator again at 3: This being done, the firſt pulling of the operator up and down will produce 1 in the window, and 7 in the ſubſtraction-ring under

under the index; and the number 1 which remain'd before in the ſyſtem C C will be changed into 0. Now, as 7 is more than 3, you muſt work on accordingly: Having done it twice more, you will find that there remains under the index *y* but 1 (which is the numerator of your fraction) and below in the two windows the quotient 13. When you conſider that diviſion is nothing elſe but a repeated ſubſtraction, you will alſo eaſily underſtand the reaſon of this operation.

For the clearer conceiving the method of proceeding with this machine in larger numbers, M. *Gerſten* explains it by the 2 following examples.

Suppoſing there are ſix ſyſtems *a*, *b*, *c*, *d*, *e*, *f*; let all the numbers pointed to by the indexes *w w* be in A B; thoſe which are to be pointed to by the determinators, in C D; and thoſe which are ſeen in the windows, in E F. Firſt of all, you muſt turn all your addition-rings of the ſilver-plates, and your multiplication-plates to 0, *viz.* that under all the indexes *w w*, and in the windows nothing may appear but 0: Write the number 3563 near the determinator, in the ſyſtems *a*, *b*, *c*, *d*, and direct them accordingly: The other number 58 you muſt likewiſe write down, but under the windows in ſyſtem *a* and *b*, as you ſee in this ſcheme. Move the ſeveral operators, which are moveable, ſucceſſively as often up and down, till 8 appear below in the windows, and you will have under the indexes above 28504, the product of 3563 × 8: And ſo the numbers of the machine will appear thus.

	f	*e*	*d*	*c*	*b*	*a*	
A	0	0	0	0	0	0	B
C			3	5	6	3	D
E	0	0	0	0	0	0	F
						5	8

Next advance your multiplicand 3563 from the right to the left; that is to ſay, place the determinator in the ſyſtem *b* at 3, in *c* at 6, in *d* at 5, in *e* at 3; and reduce every number in the windows to 0, except in the ſyſtem *a*: As may be ſeen in the following ſcheme.

	f	*e*	*d*	*c*	*b*	*a*	
A	0	2	8	5	0	4	B
C			3	5	6	3	D
E	0	0	8	8	8	8	F
					5	8	

Then

	f	*e*	*d*	*c*	*b*	*a*	
A	0	2	8	5	0	4	B
C			3	5	6	3	D
E	0	0	0	0	0	8	F
					5	8	

Then pull all the operators again succeſſively in *b*, *c*, *d* and *e*, up and down, till 5 appear in the windows below, and you will find at laſt under the indexes 206654, the product of 3563 × 58.

	f	*e*	*d*	*c*	*b*	*a*	
A	2	0	6	6	5	4	B
C			3	5	6	3	D
E		5	5	5	5	8	F
					5	8	

	f	*e*	*d*	*c*	*b*	*a*	
A	2	0	6	6	5	4	B
C			3	5	6	3	D
E	0	0	0	0	0	0	F

But if you are to divide again 206654 by 3563, you muſt place the dividend above in the ſubſtraction-rings under the indexes. In the windows below every figure muſt be 0, in the ſame manner as in multiplication; and write the diviſor under the dividend, according to vulgar arithmetic, and as in the figure here annex'd.

If you direct the determinators in *e*, *d*, *c*, *b* to their numbers, and ſubſtract this diviſor by pulling up and down the operators as often as you can, you will have in the windows in *e*, *d*, *c*, *b* every where 5; but on the ſilver-plates there will remain 28504. Now advancing your diviſor from the left to the right, bringing to the windows in *d*, *c*, *b* all the cyphers 0, and working as before, there will at laſt appear on the ſilver-plates nothing at all, but below in the windows 5888; as may be ſeen in the following ſcheme.

	f	*e*	*d*	*c*	*b*	*a*	
A	0	0	0	0	0	0	B
C			3	5	6	5	D
E		5	8	8	8	8	F

And here you have only this to obſerve, that in ſuch caſes you cut off all the hindermoſt figures or numbers in E F, except that which ſtands under the firſt figure of the diviſor; what remains is your quotient.

If it be objected that this machine cannot be fitted for ſo many and long numbers, as one would pleaſe; becauſe the multiplication of ſo many ſyſtems would require too great a force for one operator to move ſo many wheels, kept by

ſprings,

Fig. I.

Fig. II.

Fig. III.

Fig. IV.

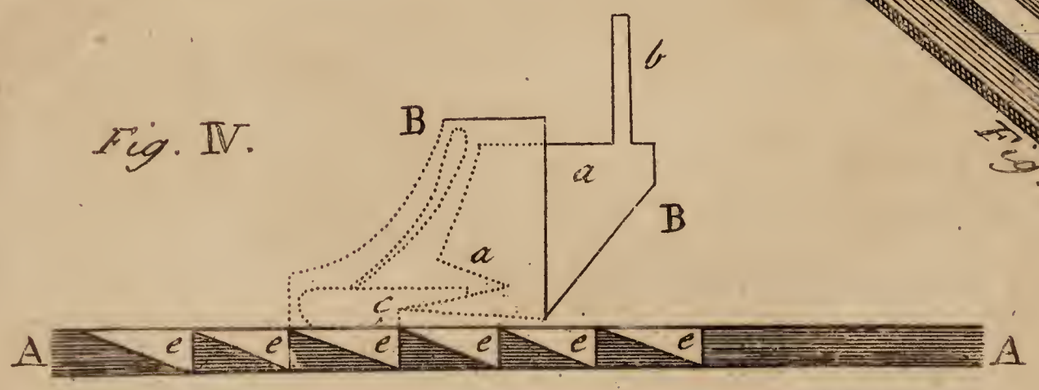
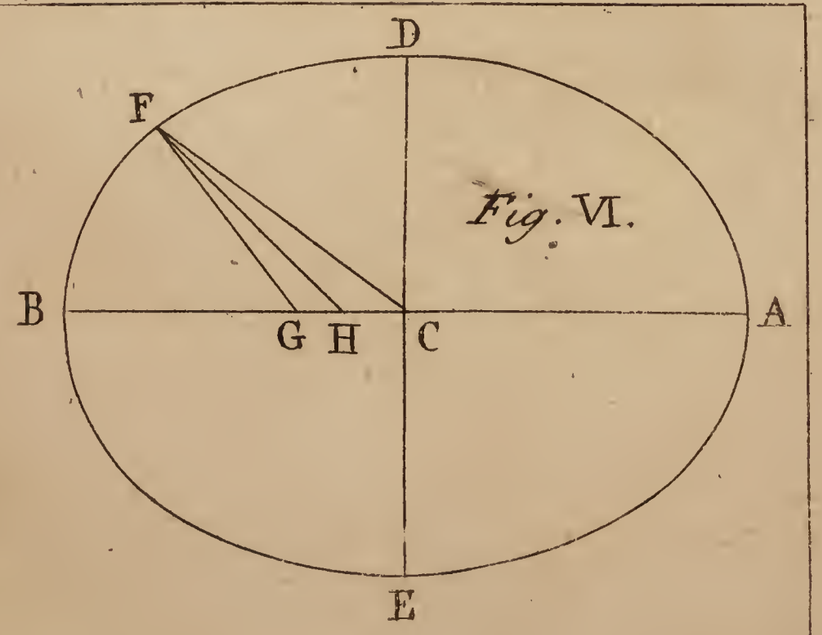
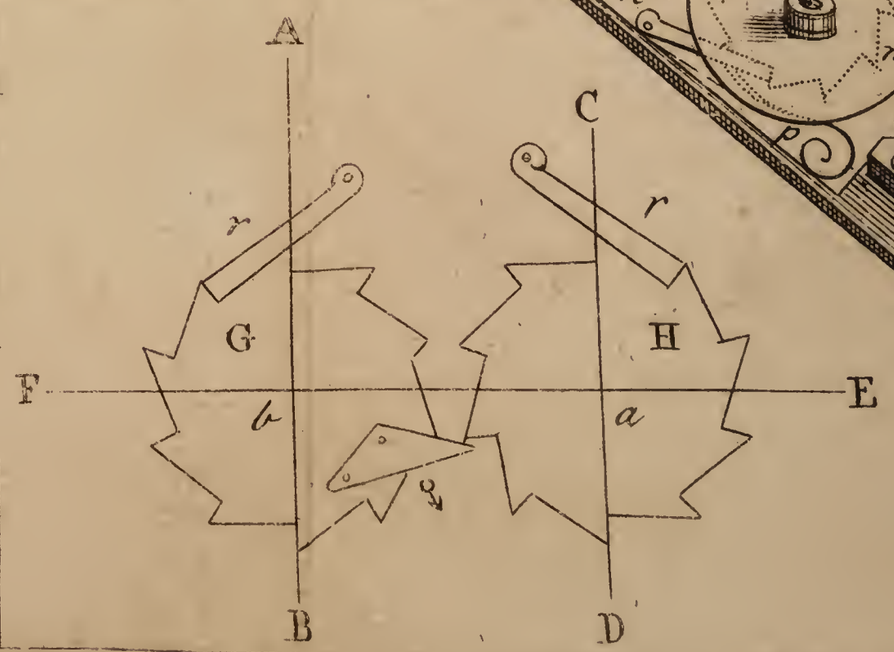


Fig. VI.





G. Smith del.

prings, ſuppoſing that all the teeth of communication ſhould uly catch; M. *Gerſten* owns that this objection is but too ell grounded: However he cannot help obſerving at the ame time, that this defect can hardly be avoided, in any rithmetical machine, for performing all thoſe operations of tſelf, without the help of the mind: For there muſt cerainly be a particular ſyſtem for each place of figures, which s to communicate with the next; conſequently, as the ſyſems increaſe in number, the force, requiſite to move them ll, muſt likewiſe increaſe. Beſides, it ought to be conſier'd, of what ſize ſuch a machine ought to be, which might erve for common uſe. M. *Gerſten* thinks few calculations ould be requir'd, for which 14 or 16 ſyſtems might not ſufice. That which he made was of 7 ſyſtems, as has been lready mention'd: The diſpoſition of it was neither ſo well contriv'd, as he has explain'd it, nor were its ſeveral parts o well executed, as a good artificer might have done; yet hoſe 7 ſyſtems were very eaſily put in motion: And if in a nachine for 14 figures, made by a ſkilful hand, it could not e ſo eaſily practicable, this defect, he believes, might be eaſily remedied, by applying the other hand in the 5th or 6th yſtem to the handle *ſs*, in order to eaſe and aſſiſt the opeator.

Of the Figure of the Earth *and the Variation of Gravity on the Surface; by Mr.* James Stirling. Phil. Tranſ. N° 438. p. 98.

THE centrifugal force, ariſing from the diurnal rotation of the earth, depreſſeth it at the poles, and renders it protuberant at the equator, as has been lately advanced by Sir *Iſaac Newton*, and long ago by *Polybius*, according to *Strabo* in the ſecond book of his *Geography*: But tho' it be of an oblate ſpheroidical figure, yet the kind of that ſpheroid has not hitherto been diſcover'd; and therefore Mr. *Stirling* ſuppoſes it to be the common ſpheroid, generated by the rotation of an ellipſis about its leſſer axis; tho' he find by computation, that it is only nearly, and not accurately ſuch. He likewiſe ſuppoſes the denſity to be every where the ſame from the centre to the ſurface, and the mutual graviration of the particles towards one another to decreaſe in the duplicate ratio of their diſtances: And then the following rules will follow from the nature of the ſpheroid.

1. Let ADBE (Fig. 6. Plate V.) be the meridian of an oblate ſpheroid; DE the axis; AB the diameter of the equator, and C the centre: Take any point on the ſurface, as F, from which draw FC to the centre, FG, perpendicular to the ſurface at F, meeting CB in G, and FH cutting the line CG; ſo that CH may be to GH as 3 to 2. A body at F will gravitate in the direction FH; and that the mean force of gravity on the ſurface will be to the exceſs of the gravity at the pole above that at F, as the mean diameter, multiplied into the ſquare of the radius, is to one fifth of the difference of the longeſt and ſhorteſt diameters, multiplied into the ſquare of the co-ſine of latitude at F.

2. The decrement of gravity from the pole to the equator is proportional to the ſquare of the co-ſine of latitude; or which comes to the ſame thing, the increment of gravity from the equator to the pole is proportional to the ſine of latitude. Hitherto Mr. *Stirling* has conſider'd the variation of gravity, which ariſes from the ſpheroidical figure, while it does not turn round its axis: But if it doth, the direction of gravity will be in the line FG, perpendicular to the ſurface; and its variation now ariſing both from the figure and centrifugal force, will be 5 times greater than what ariſes from the figure alone; as will appear from the proportion of the lines FH and FG, the former being to the latter, as the whole force of gravity at F, while the ſpheroid is at reſt, to the force with which a body deſcends at F, while it turns round its axis.

3. From this laſt article it appears, that one fifth of the variation of gravity is occaſion'd by the figure of the ſpheroid, and the remaining four fifths by the centrifugal force. And whereas the earth could not be of an oblate ſpheroidical figure, unleſs it turned round its axis; nor could it turn round its axis, without putting on that figure: The diminution of gravity towards the equator, known by the experiments with pendulum's, prove both the rotation and oblate ſpheroidical figure of the earth.

4. The mean force of gravity on the ſurface is to the centrifugal force at any point F, as a rectangle under the radius and mean diameter to a rectangle under the co-ſine of latitude, and four fifths of the difference of the longeſt and ſhorteſt diameters. And at the equator, where the co-ſine of latitude becomes equal to the radius, the mean force of gravity is to the centrifugal force, as the mean diameter to four

fifths

fths of the longeſt and ſhorteſt diameters. This article is ound from the proportion of the lines F H and G H; the ormer being to the latter, as the force of gravity to the centrifugal force.

5. The proportion of the diameters of the earth will be ound in the following manner. The moon revolves about the arth in 27d 7h 43′, or in 39343 minutes; and her mean iſtance is about 59 ½ ſemidiameters of the earth, according o *De la Hire*'s and *Flamſtead*'s tables, but near 60 ½ ſemiiameters by *Halley*'s. Mr. *Stirling*, therefore, takes 60 for he mean diſtance, till it be better known: Then according o the nature of gravity, as the cube of the moon's diſtance o the ſemidiameter of the earth, or as 216000 to unity, ſo is 547870000, the ſquare of the periodic time of the moon to 166, the ſquare of the number of minutes, in which another moon would revolve about the earth at the diſtance of ts ſemidiameter: And as this laſt number to 2062096, the quare of 1436, the number of minutes in a ſidereal day, ſo s unity to 287.7; which would ſhew the proportion of the entrifugal force at the equator to the mean force of gravity by *Corol.* 2. *Prop.* 4. *lib.* 1. *Princip. Newton.*) were it not for the action of the ſun on the moon: Therefore (by *Corol.* 17. *Prop.* 66. *lib.* 1. *Princip.*) as the ſquare of the ſidereal year to the ſquare of the periodic time of the moon, that is, as 179 to unity, ſo is 287.7 to 1.6, which being added o 287.7, makes 289.3. And therefore as unity to 289 (neglecting the fraction which is uncertain) ſo is the centrifugal force at the equator to the mean force of gravity on the ſurface. And thence (by article 4.) as 289 to ¼, ſo is the mean diameter to the difference of the longeſt and ſhorteſt: And therefore, as the axis is to the equatorial diameter; ſo is 2307 to 2317; or in ſmaller numbers, as 231 to 232, the ſame as Sir *Iſaac Newton* found in a different manner: For, he makes it as 230 to 231; and as 230 to 231, ſo is 231 to 232.004.

6. In the ſame manner the proportion of the diameters of any planet may be found, if it have a ſatellite: For inſtance, in *Jupiter*, he turns about his axis in 9h 56′, or in 596 minutes: And his third ſatellite revolves about him in 7d 3h 42′ 36″, or in 10302.6 minutes, at the diſtance of 15.141 of his ſemidiameters. Therefore, as the cube of 15.141 to unity, ſo is the ſquare of 10302.6 to 30579, the ſquare of the number of minutes, in which a ſatellite would revolve about him

at the distance of his semidiameter. And as this last number is to 355216, the square of 596, so is unity to 11 $\frac{5}{8}$, or the centrifugal force at his equator to the mean force of gravity on his surface. There is no need of correcting this number, as in the former article; because the periodic time of *Jupiter* round the sun is vastly greater than that of his third satellite round him. Mr. *Stirling* has chose the third satellite before any of the rest; because its greatest elongation was observed by Dr. *Pound*, with a micrometer adapted to a telescope 123 foot long; and he also took the diameter of *Jupiter* by the transit of the satellite, which is a much more exact way than with a micrometer. But as the planes of *Jupiter*'s satellites almost coincide with the plane of his equator, the diameter, determin'd by the transit of the satellite, is his greatest; and the distance of the satellite, which ought to have been given in his mean diameters, is assigned in his greatest: For which reason the force of gravity, already found, must be augmented in the triplicate ratio of his greatest diameter to his mean one; that is, if a represent the mean diameter, and d the difference of the longest and shortest, in the proportion of $2a + 3d$ to $2a$ very nearly. Hence, as the centrifugal force at his equator, to the mean force of gravity on his surface, so is unity to $11\frac{5}{8} \times \frac{2a+3d}{2a}$. And (by art. 4.) $11\frac{5}{8} \times \frac{2a+3d}{2a} : 1 :: a : \frac{4}{5}d$, or $20aa = 186ad + 279dd$; which makes a to d, as 108 to 10: And thence the axis is to the equatorial diameter, as 108—5 to 108 + 5, or as 103 to 113; that is, as 12 to 13 $\frac{1}{6}$; which accurately agrees with the observations both of Dr. *Pound* and Mr. *Bradley*, made with *Huygens*'s long telescope; the former making it as 12 to 13; and the latter as 25 to 27, which is very nearly the same. And if this theory agree so well with observations in *Jupiter*, there is no doubt but it will be more exact in the earth, whose diameters are much nearer to equality.

7. By experiments made at *Jamaica* (vide *Phil. Trans.* N° 432. p. 302.) in Lat. 18. with a very curious clock, contriv'd by Mr. *Graham*, it was found that the *London* pendulum went slower there by 2′ 6″ in a sidereal day, than at *London*. But it was found by experiments made with thermometers, that 9″ were to be allowed for the lengthening of the pendulum by heat: and therefore it was retarded only 1′ 57″ by the decre-

ment

ment of gravity. So that while a pendulum at *London* makes 86164 vibrations, the number of ſeconds in a ſidereal day, the ſame at *Jamaica* does only give 86047 vibrations. Therefore the force of gravity at *London* is to that in the latitude of 18 degrees, as the ſquare of 86164 to the ſquare of 86047; that is, very nearly as 1106 to 1103. And (by article 1, 2,) if a denote the mean diameter of the earth, d the difference of the greateſt and ſmalleſt $a - \frac{ccd}{rr}$ will denote the force of gravity in general in any latitude, whoſe coſine is to the radius as c to r: Where if in the place of c there be ſubſtituted the coſines of 51° 32′ and 18°; that is of the latitudes of *London* and *Jamaica*, we ſhall have the force of gravity at the former to that at the latter, as $a - |387\,d$ to $a - |9045\,d$; that is, as 1106 to 1103. Whence the mean diameter of the earth will be to the difference of the axis and equatoreal diameter, as 191 to 1; and thence (by article 4.) as the mean gravity on the ſurface to the centrifugal force at the equator, ſo is 191 to $\frac{4}{5}$, or ſo is 239 to unity. In order to ſhew that this cannot be, Mr. *Sterling* obſerves, that when the moon's diſtance was ſuppoſed 60 ſemi-diameters of the earth (as in art. 5.) it was found that the mean force of gravity was to the centrifugal force at the equator, as 289 to 1. But if the proportion now found be true, the moon's diſtance at 60 ſemi-diameters muſt be augmented in the ſubtriplicate ratio of 289 to 239; and then it will become 64 ſemi-diameters. In like manner, if we compute the ratio of the mean force of gravity to the centrifugal force, by preſuppoſing the magnitude of the earth, as Sir *Iſaac Newton* and M. *Huygens* did, we muſt ſuppoſe a degree to be upwards of 80 *Engliſh* miles to bring it out 23. 91. Now whereas it is certain that the diſtance of the moon is about 60 ſemi-diameters of the earth, and that a degree is leſs than 70 *Engliſh* miles; therefore the concluſion, which ſeems to follow from the *Jamaica* experiment, cannot be allow'd to be true. And the experiments made by *Richer*, in the iſland of *Cayenne*, wou'd ſtill make a greater difference betwixt the diameters of the earth, than thoſe made in *Jamaica*: And the lengths of the *Paris* and *London* pendulums, compared together, would make it greater than $\frac{1}{231}$ part of the whole, as it was found in article 5.

8. From all the experiments made with pendulums, it appears, that the theory makes them longer in iſlands, than they are

are found in fact. The *London* pendulum ſhould be longer, when compared to the *Paris* one, than it really is: The *Jamaica* pendulum when compar'd to the *London* one, which vibrates in a greater iſland, ſhould be longer than is found by experience; and the pendulum in *Cayenne* (a ſmaller iſland than *Jamaica*) ſhould ſtill be longer. This defect of gravity in iſlands is very probably occaſioned by the vicinity of a great quantity of water, which being ſpecifically lighter than land, attracts leſs in proportion to its bulk. And Mr. *Stirling* finds by computation that the odds in the pendulums betwixt theory and practice is not greater than what may be accounted for on that ſuppoſition. He alſo obſerves, that tho' the matter of the earth were entirely uniform, yet the hypotheſis of its being a true ſpheroid is not near enough the truth to give the number of vibrations, which a pendulum makes in 24 hours. And ſuppoſe the true figure were known, the inequalities of mountains and valleys, land and water, heat and cold, would never allow theory and experiments to agree. But after the *French* Gentlemen, who are now about meaſuring a degree, and making experiments with pendulums in the north and ſouth, ſhall have finiſhed their deſign, we may expect new light in this matter.

Of the Mexican *filtre Stone*; *by Dr.* Abraham Vater. Phil. Tranſ. N° 438. p. 106. Tranſlated from the *Latin*.

THIS Stone has got the name filtre from its poroſity, whereby it lets liquors paſs thro' it; and for this reaſon pots and mortars are made from larger pieces of it, to ſtrain liquors, particularly water to drink: For, it is thought, that the water filtred thro' this ſtone is thereby freed from all its impurities, and becomes clearer and purer, and more wholſome. This is the reaſon that theſe ſtones are highly valued in *Japan*, and ſold at the price of gold, becauſe the *Japaneſe*, who know nothing of the ſtone or any other diſorder in the kidneys, and who prefer health far before all other bleſſings, are of opinion, that theſe petrified *fungi* have the power of prolonging life, as may be ſeen in *hiſtoria filtri, lapidis in Valentini Muſ. Muſeor. lib.* 1. *cap.* 22: For, this ſpecies of *fungus*, as related in *loc. citat.* grows on the rocks in ſome places of the Gulph of *Mexico*, about 100 elns under water, and ſpontaneouſly hardens and petrifies in the air. Dr. *Vater* will not take upon him to determine the origin of the filtre ſtone, nor manner of its growth, tho' both appears to be very ſuſpicious, and invented purely to prevent its being thought a common ſtone.

ſtone. For, *Lentilius, in ephemer. German. cent.* III. *Obſ.* 176, writes that there are veſſels made of two kinds of it; one of a dark grey colour like the *lapis Sciſſilis* from *Canada*, a province in *America*, and ſold at a dearer rate; and others of a tophaceous colour, and of the growth of *Italy*. Nay, according to *Le Clerc* in his phyſics, as appears by a note annexed, it is likewiſe dug up in the biſhoprick of *Liege*, and of great uſe in *Holland*: Dr. *Ehrhart* of *Memmingen*, preſented Dr. *Vater* with a choice collection of foſſiles, among which was a *tophos* peculiarly porous, found about *Memmingen*, and which, as he aſſured him, would ſtrongly imbibe water, whatever part of it was immers'd therein: For, no ſooner is the ſurface of the water touch'd by it, but by the preſſure of the atmoſphere, the water aſcends, and is carried quite thro' its porous ſubſtance, as we find by experience, in ſugar, ſalt, filtring paper and ſponge. And, this immediately ſuggeſted the hint, whether it might not be made uſe of inſtead of the *Mexican* filtre to ſtrain water. In order to try this, he made a hollow in a little bit of it, and upon pouring water therein, he ſaw it ſtrain very faſt thro' its pores. He therefore propoſed to try the ſame thing with other *tophi* and ſtones; and with this deſign he took the tophaceous tubes of oſteocolla, and ſtopping one extremity, he poured water thereon; and it tranſuded very faſt thro' its porous ſubſtance. He moreover recollected that he had a ſponge for ſeveral years, which when he lived at the *Caroline* bath, he had put in a pipe that convey'd the hot waters, and by this means the ſponge being incruſtated with the ochre, which the hot waters carry along with them and depoſite in their paſſage, degenerated into a *tophus*: He made a pit in the ſponge, and fill'd it with water, and it ran very faſt through it. Upon this he reſolv'd to make trial with a *tophus* of the hot bath, of which he had a pretty large piece; and for this purpoſe he gave it a ſtone-cutter to hollow into a mortar, to ſee whether the water would paſs thro' that denſe and ſolid ſtone: And it anſwer'd his expectation: For, upon pouring water therein it ſtrain'd through in the ſame manner as through the *Mexican* filtre and other *tophi*; but by reaſon of the denſity of the ſtone, ſlower than through more porous ſtones. The Dr. was pleaſed that he had by this experiment diſcover'd ſomething that might contribute to illuſtrate the generation of the *Mexican* filtre under water: For, ſince the *tophus* of the hot baths is generated from the water depoſiting its ochre, in flowing thro' the pipes, and is inſenſibly con-

concreted; he thought in like manner that the ſea beating upon the rocks depoſits ſaline earthy particles, from whoſe ſucceſſive concretion this ſtone is generated, and rather grows on the rocks, than like rock-muſhrooms, ſprings from them; but ſince the origin of this ſtone, as has been ſhewn above, is very doubtful, and it does not manifeſtly appear, whether it be taken from the bottom of the ſea, or rather dug out of the earth, the Dr. will not take upon him to determine any thing about it: But conſidering the remarkable denſity of the hot bath *tophus*, thro' which notwithſtanding water filtrates, he had a mind to try the ſame experiment with the common ſtone made uſe of in building. The ſucceſs anſwer'd expectation: For, a mortar made of ſuch ſtone ſerved inſtead of the *Mexican* filtre, the water ſtraining equally clear thro' both. The water ſtrained in this manner acquir'd at firſt an earthly taſte, which yet upon repeated filtration it loſt; as *Lentilius* in *loc. ſupra citat.* obſerv'd of the ſtone filtre: There is likewiſe no doubt, but that other ſtones have the ſame effect; becauſe the hardeſt and moſt ſolid flint ſtones, with which ſtreets are paved, imbibe water, as we obſerve in rainy weather: Yet it is evident to any one, that the more ſolid and denſe the ſtones are, and the narrower their pores, the water pour'd on them will the more difficultly and ſlowly pervade them:

But now we come to the virtue aſcribed to this ſtone-filtre, as that water ſtrained thro' it is freed from all its impurities: For, thus we read in the above-cited hiſtory *in Valentini muſæo*, that water filtred thro' it, tho' very clear when pour'd therein, yet always depoſites ſome little imperceptible dregs, and becomes ſenſibly lighter, purer, and more wholeſome; nay keeps a much longer time without turning muſty. Were this the caſe, and could water be freed by this means from all heterogeneous particles, ſuch filtres would deſervedly be very valuable: For, what conduces more to health than pure water in meat and drink. And yet vaſt tracts of the earth are deprived of this bleſſing, where they have none, but what is brackiſh, nitrous, vitriolic and aluminous, and impregnated with other mineral particles, which may occaſion ſeveral diſeaſes. It ſeems entirely probable that water, ſtrain'd thro' ſuch ſtone-filtres, depoſites theſe impurities; becauſe we ſee, and experience confirms it, that ſprings bubbling up in ſandy, gravelly places, and ſtraining thro' ſuch a bottom, are much more limpid and pure than other ſprings: But the purity of theſe waters is not owing to their filtrating

thro'

hro' the ſand and gravel and depoſiting their impurities therein, but rather to their carrying none along with them: For, it is verified by chemical experiments, that not only ſalts, but likewiſe by means thereof other heterogeneous, earthy, ſulphureous, and mineral particles, diſſolv'd in the waters, and cloſely united with them, can by no means be ſeparated from them by filtration: For, we obſerve that the moſt ſolid minerals, as mercury, antimony, lead and other minerals diſſolved, paſs thro' the pores of a paper filtre, and are afterwards ſeparated by precipitation from the waters, and by repeated filtration remain in the filtre: But, it may be objected that a coarſe and denſe ſtone-filtre is of greater efficacy in this caſe than a paper one: Yet the contrary appears from the waters that exſudate and diſtil from mines and ſubterraneous cavities, which immediately petrify, whence ariſes the ſtalactites. From this alone it appears, of what little uſe the moſt ſolid ſtones, and conſequently, ſtone filtres are, to depurate water, and ſeparate the ſaline earthy and mineral impurities, diſſolv'd therein. Yet the Dr. does not deny, but that muddy and ſlimy waters may by ſtraining thro' ſuch ſtones become clear and pellucid; becauſe theſe impurities do not diſſolve in the water or intimately incorporate therewith, but only float therein. But beſides theſe, no other waters can by any means become purer, as he learned from repeated experiments, both with the filtre from *Holland*, and with thoſe made from the *tophus* of the Caroline hot baths and common ſtone, on ſeveral kinds of river and ſpring water; and with an hygrometer examining their weight both before and after filtration, he found little or no difference. How happy, therefore, are the countries, to whom a kind providence has given limpid and wholeſome ſprings of water, that require no ſuch filtres. But ſuch as are deprived of this bleſſing, can do no better than uſe rain-water, as being by diſtillation freed of all its impurities; and conſequently the pureſt and moſt defecated.

A Halo *obſerved at* Rome Aug. 11. 1732; *by* S. De Revillas. Phil. Tranſ. N° 438. p. 118. *Tranſlated from the* Latin.

FROM 3 o'clock in the forenoon till 2 in the afternoon a ſimple halo, every way defined, and exactly circular, was obſerv'd to ſurround the ſun in its pole; its breadth ſeem'd to equal the apparent diameter of the ſun; the innermoſt colour was red, the reſt pretty dilute, and analogous to

thoſe obſerved in the rainbow, but terminating in a whitiſh brightneſs, and ſomewhat changed about noon. Tho' the ſky was ſerene, it was tinged with a thin darkneſs, a north wind blowing very gently. Afterwards the darkneſs condenſed into whitiſh little clouds, while the halo vaniſh'd, whoſe diameter meaſur'd from the innermoſt edge of the zone 45 degrees.

The height of the barometer the ſame day was, as follows;

At 4 o'clock in the forenoon	—	27.	11 *Paris* inches
At noon — — —		27.	10 $\frac{1}{2}$
At 2 in the afternoon — —		27.	10 $\frac{1}{2}$.

An Account of an ancient Date *found at* Widgel-hall *in* Hertfordſhire; *by Mr.* John Cope. Phil. Tranſ. N° 439. p. 119.

FIG. 1. Plate VI. repreſents an ancient chimney-piece (as Mr. *Cope* was informed) diſcover'd on pulling down part of *Widgel-hall* in *Hertfordſhire*: There is cut upon it a date, partly expreſs'd in *Roman* numerals, and partly in *Indian* figures; which is the earlieſt inſtance he has met with of the *Indian* figures being uſed in *England*, *viz.* M. 16, or 1016; that at *Colcheſter* being *An.* 1090. vide *Phil. Tranſ.* N° 266. The carving is very fair, the letter M and the figures project out above $\frac{1}{4}$ of an inch. The whole chimney piece is of *Engliſh* oak-plank; and was very firm, tho' 718 years old, and never painted over; it is 4 foot 3 inches and $\frac{1}{2}$ long: The part under the figures 16 was broken off in taking it down in *Aug.* 1733, when the houſe was on fire.

Remarks on the aforeſaid ancient Date, found on an Oaken Plank *at* Widgel hall *near* Buntingford *in* Hertfordſhire, *now preſerv'd in the* Muſæum *of the* Royal Society; *by Mr.* John Ward. Phil. Tranſ. N° 439. p. 120.

APRIL 4, 1734, a curious draught of an ancient date, carv'd in an oaken-plank at *Widgel-hall*, the ſeat of Mr. *Gulſton*, was laid before the *Royal Society*, as the moſt early inſtance of our common figures, uſually call'd *Arabian*, which had ever been obſerv'd in *England*. It was read M 16 and thought to expreſs the year 1016: the M being taken for a *Roman* numeral, and the 16 for *Arabian* figures.

Dr. *Wallis* had in 1683 communicated to the *Royal Society* the draught of a mantle tree, ſomewhat like this, which he ſaw

Fig I

A TABLE OF CHARACTERS

	unus	duo	tres	quatuor	quinque	sex	septem	octo	novem	decem	
Tyro and Seneca's Notes:											From Gruter's Antiquitat. Vol. II in fin
	primus	secundus	tertius	quartus	quintus	sextus	septimus	octavus	nonus	decimus	
	semel	bis	ter	quater	quinquies	sexies	septies	octies	nonies	decies	
Boethius's apices.											Doctor Meads Manuscript.
Small Greek letters.	α	β	γ	δ	ε	ς	ζ	η	θ	ι	
Modern Indian figures											Tavernier Liv. 1 chap. 2
Arabic figures	1	2	3	4	5	6	7	8	9	1	Manuscripts
Figures of Jo de Sacro Bosco.	1	2	3	4	5	6	7	8	9	10	Doctor Wallis
Figures of Max. Planudes	1	2	3	4	5	6	7	8	9	10	
Figures in Rog. Bacon's Calendar.	1	2	3	4	5	6	7	8	9	10	The Cotton Library
Modern fig.	1	2	3	4	5	6	7	8	9	10	

F. III. F. IV. F. V.

Fig. VI.

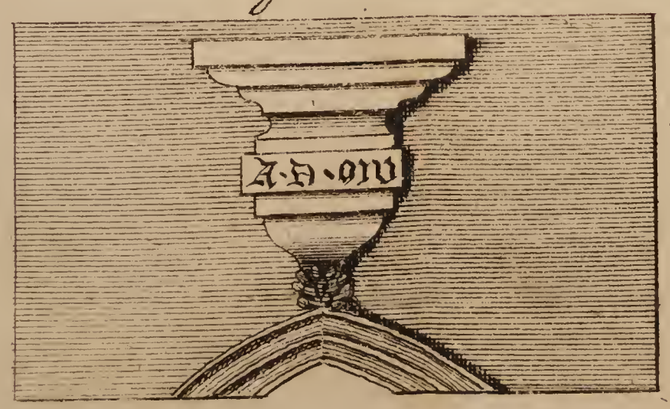


Fig. VIII. F. IX. F. X.

F. XI.

F. XII.

Fig. VII

Indian	Antient	Modern
1	1	1
2	2	2
3	3	3
4	4	4
5	5	5
6	6	6
7	7	7
8	8	8
9	9	9
0	0	0

F. XIII. F. XIV. F. XV. F. XVI. F. XVI

F. XVIII. F. XIX. F. XX. F. XXI. F. XXII

Fig XXIII

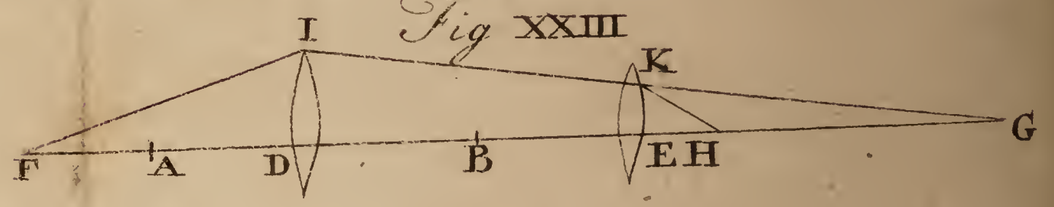
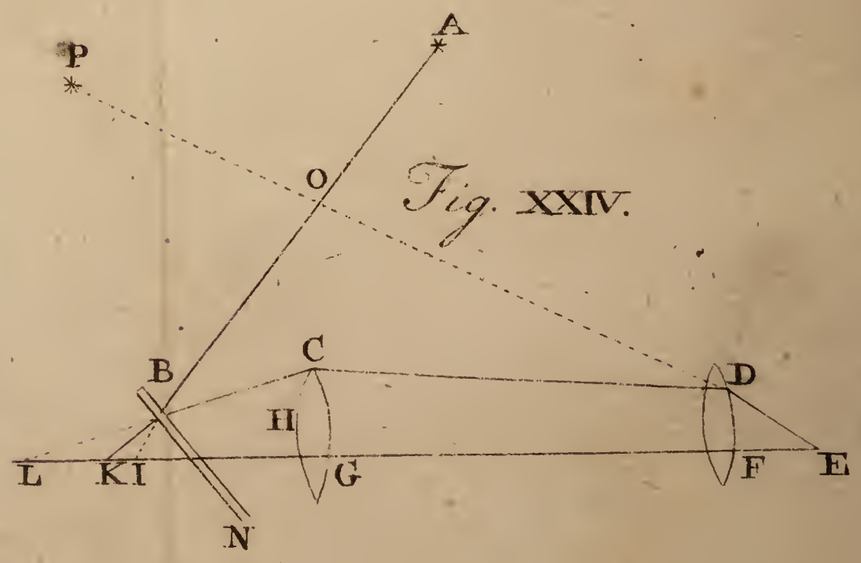


Fig. XXIV.



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saw at the parsonage-house at *Helmdon* in *Northamptonshire*. The date, which was likewise carv'd in mix'd characters expres's'd the year M 133, as the Dr. read it: This being the oldest monument of that sort, which had then been discover'd among us, was first publish'd in *Phil. Trans.* N° 154, and afterwards in the Dr's *Algebra, cap.* 4. *p.* 14.

An. 1700, another draught of a date at *Colchester*, which had been sent to Dr. *Wallis* by Mr. *Luffkin* (who copied it from the under cell of a wooden window, and read the figures 1090, being all *Arabian*) was likewise publish'd in the *Phil. Trans.* N° 266, as more ancient than the former.

None earlier than these 2 last had hitherto appear'd, till that from *Widgel-hall*: Upon the sight of which Mr. *Ward* thought the reading given to it look'd very plausible. The mixt characters were no just objection, which Dr. *Wallis* had accounted for in the *Helmdon* date *Phil. Trans.* N° 266, and Mr. *Ward* himself observed in some manuscripts: But still one difficulty seemed to remain, which was the want of some character in the place of hundreds: And therefore, soon after going into *Hertfordshire*, he took that opportunity to wait upon Mr. *Gulston*, in order to see the original, which was some time after presented to the *Royal Society*, together with a letter giving an account of the antiquity of the building in which it stood. And as that letter may afford some light to the enquiry about the date cut in the plank, it is as follows.

Part of a letter from *Francis Gulston* Esq; to Mr. *John Ward*.

Sir,

'I can give you no further account of the antiquity of the building, than that in general it was esteemed ancient. Before the house was burnt, on the timbers there were several old coats of arms; some we look'd on as belonging to the family of the *Scalers*, who were possessors of *Widdihale* with other estates soon after the conquest.

" *Widdihale* in *Hertfordshire* in the time of the Conqueror was parcel of the estate of *Hardwin de Scalers*, as appears by *Domesdaibook*, fol. 141. It continued in that family for several generations, till it came to *Anthony Widvile*, by the marriage of the daughter and heir of *Scalers*. But when he would not comply with *Richard* III. to destroy the young Princes, all his lands were seised, and the Manor continued in the Crown, till *Henry* the VIII.

'granted it to *George Canon* and *John Gill*: *George Gill*, 'the ſon of *John*, marrying the daughter of *George Canon*, 'obtain'd the whole. In this family it continu'd till the be- "ginning of the reign of *James* I. when it was ſold to *John* "*Goulſton* Eſq; whoſe deſcendants now hold it. *vide* Sir "*Henry Chauncy's hiſt. and antiquit. of Hertfordſhire*, "p. 111."

'The houſe of *Widdihale*, probably, might have been of 'greater antiquity, and I believe really was: For, at the 'time of the conqueſt it was in the poſſeſſion of a conſider- 'able follower of *Harold*.

'The piece of timber I ſend you was the top of a door- 'way, in a timber-built houſe, and plaiſter'd over with mor- 'tar. From the date on the plaiſter'd wall, the door had not 'been us'd at leaſt 344 years: For, on the outſide was 'plainly to be ſeen the date 1390. Part of the room this 'was found in was burnt too much to repair again: And in 'taking down the burnt timbers, being preſent myſelf, I ac- 'cidentally ſaw it, and obſerving the date, thought it a cu- 'rioſity, that might give to the curious ſome ſpeculation.'

I am, &c.

'*Woodbridge* in *Suffolk*
'*July* the 14, 1734.'

Francis Gulſton.

Upon conſidering the characters on this plank, and thoſe of the other 2 dates mention'd above, together with the accounts given by learned men of the time, when the *Arabian* figures were firſt introduced into theſe parts of the world, and the various forms they have ſince receiv'd, as repreſented in Fig. 2. Mr. *Ward* was at laſt ſatisfied, that none of theſe 3 dates prove they were ever us'd among us, in leſs than 100 years after the reading given to the lateſt of them. And the reaſons which led him into this opinion, he now begs leave to offer, after having firſt briefly enquir'd into their origin and antiquity.

Moſt writers, who have treated of the riſe of theſe figures have thought that they came firſt from the *Perſians* or *Indians* to the *Arabians*; and from them to the *Moors*; and ſo to the *Spaniards*, from whom the other *Europeans* receiv'd them. This was the opinion of *John Gerard Voſſius*, de Naturâ art. lib. 3. cap. 8. § 6. Mr. *John Greaves de ſiglis Arabum & Perſarum aſtronom.* p. 2. where the form of them may be ſeen.

Biſhop

Biſhop *Beveridge*, *Arithmet. Chronolog. lib.* 1. *cap.* 5. Dr. *Wallis*, *de Algebra cap.* 3. *p.* 10. and many others. And the *Arabians* themſelves own they had them from the *Indians*; as both Dr. *Wallis ibid.* p. 9. and Mr. *Greaves*, *de ſiglis Arabum*, &c. have ſhewn from their writers.

But *Iſaac Voſſius* thought the ancient *Greeks* and *Romans* were acquainted with theſe figures; and that the *Arabians* took them from the *Greeks*, and the *Indians* from the *Arabians*, *Obſervat. ad Pomp. Mel.* p. 64. For the proof of this he refers to *Tyro* and *Seneca*'s notes, *Grut. inſcript. Vol. II. ad fin.* and *Boethius*'s treatiſe *de geometria*, *lib.* 1. *ſub fin.* But as to the notes of *Tyro* and *Seneca*, they ſeem to have no affinity with theſe figures, either in their number or nature: For, they are not limited to 9, but many times that number, and all different in form. Nor are they ſimple ſigns of numbers, but complex characters of ſeveral letters of thoſe numeral words, which they ſtand for in the *Roman* language, like our ſhort-hands; and therefore vary in their ſhape, as they are deſigned to expreſs cardinals, ordinals, or adverbs of number. This will appear by the table of characters (Fig. 2.) in which are repreſented the firſt ten of each. But as to what *Voſſius* ſays concerning *Boethius*, Mr. *Ward* obſerv'd in a curious manuſcript of that writer, now in Dr. *Mead*'s library, 9 characters, which he tells us were invented and uſed by ſome of the *Pythagoreans* in their calculations; while others of them made uſe of the letters of the alphabet for the ſame purpoſe. *Boethius* calls them *apices* or *characteres*, *ubi ſupra:* Theſe are alſo inſerted in Fig. 2. to ſhew the great affinity between them and the *Arabian* figures, as theſe latter were written 2 or 3 centuries ago.

The opinion of *Daniel Huetius* differs from either of the former: For in *Demonſtrat. evangel. prop.* iv. c. 13. p. 172. he imagined, that the *Arabian* figures were only the letters of the *Greek* alphabet, corrupted and alter'd by ignorant *Librarians*.

From this ſummary account of the riſe and antiquity of theſe figures, it ſeems probable to Mr. *Ward*, they might owe their original to the *Greeks* (thoſe common Maſters of all ſcience) and paſſing from them firſt to the eaſtern nations, come round to theſe weſtern parts, in the manner before deſcribed. We have no other author who ſpeaks of this matter, near ſo ancient as *Boethius*, whoſe words are very expreſs, and greatly ſtrengthen'd by the ſimilitude of his characters with

with the *Arabian* figures. And therefore, we may rather ſuppoſe, they took their riſe from theſe than from the ſmall *Greek* letters, with which *Huetius* compared them; ſince theſe latter are neither ſo like them, nor ſo old as the time of *Boethius*. And tho' what the *Arabians* ſay may be true, that they had them from the *Indians*, and not the *Indians* from them, as *Iſaac Voſſius* conjectur'd: Yet it may be equally true, that the *Indians* had them firſt from the *Greeks*, and thoſe *Arabian* writers (who are not very ancient) not have known it; nor are there any *Indian* monuments of ſufficient antiquity to render this opinion queſtionable.

But whichſoever of theſe ſentiments may be eſteemed the moſt credible, with reſpect to the origin of theſe figures; *Joſeph Scaliger lib.* 3. *ep.* 113. thought they were not receiv'd by the *Europeans*, as they came of later ages from the *Arabians*, long before the year 1300.

But *John Gerard Voſſius de natur. Art. lib.* 3. *cap.* 8. § 7. was of opinion they began to uſe them about the middle of the thirteenth century, or the year 1250.

F. *Mabillon*, in his treatiſe *de re diplomatica*, was neceſſarily led to attend to the uſe of theſe figures, particularly in dates. And he informs us, that they were rarely uſed before the fourteenth century, except in ſome few books of geometry and arithmetic. And preſently after he ſays, *lib.* 2. *cap.* 28 § 10. it was not much to his purpoſe to treat of them; ſince he did not deſign to carry his work lower than the thirteenth century. By which he ſeems to intimate, that he had met with very few, if any, inſtances of *Arabian* figures, in ſuch inſtruments at leaſt, before the year 1300.

But no one appears to have examin'd this ſubject more carefully than Dr. *Wallis*, who has offer'd ſome arguments to prove, that *Gerbertus*, a monk, who was afterwards advanced to the Papal ſee, and took the name of *Sylveſter* II. had before the year 1000 learn'd the art of arithmetic, as now practis'd, with the uſe of 9 characters only (whatſoever their form then was) from the *Saracens* in *Spain*, which characters he afterwards carried into *France*, *de Algebra* c. 4. p. 17. But the Dr. thinks thoſe characters or figures were known for a long time after only to ſuch artiſts, and principally uſed by them in aſtronomical calculations; the *Roman* numerals being ſtill retained in common uſe to expreſs ſmaller numbers, *ibid.* *p.* 11, 15, 16. Nor has he given us the figures us'd by any of thoſe writers, before *Johannes de Boſco ſacro*, who died in the

the year 1256; and *Maximus Planudes*, a *Greek*, who flouriſh'd after him; which Mr. *Ward* has copied from him, and inſerted in Fig. 2.

Mr. *David Caſley*, in his *Catalogue of the Manuſcripts of the King's Library*, &c. has publiſh'd a ſpecimen of manuſcripts from the *Cottonian* library, call'd *Calendarium Rogeri Bacon*, Pl. XV. and dated 1292. The figures in this book are *Arabian*; and as Mr. *Caſley* informed him, the oldeſt that he remembers to have met with in either of thoſe libraries: For which reaſon Mr. *Ward* has given them a place in Fig. 2.

It appear'd to Mr. *Ward* exceeding difficult, how to reconcile the opinions and obſervations of theſe ſeveral writers, concerning the firſt uſe of the *Arabian* figures in theſe weſtern countries, with the time aſſigned even to the lateſt of the dates above-mentioned: And it could not but ſeem very ſtrange, that no date of any writing ſhould have been produced in thoſe figures, or any other uſe of them diſcovered (except perhaps in ſome mathematical calculations, or books of arithmetic) long before the fourteenth century: And yet that a date ſhould be found, ſo carv'd in a piece of wood, before the middle of the twelfth century, for ſo common a purpoſe as the mantle-tree of a chimney.

But upon a cloſer examination of the characters he found reaſon to think, this was not really the caſe; and that inſtead of 1133, they ought to be read 1233, what has been taken for 1, being deſigned for a 2. This reading ſeems to be confirmed by the ſhape of the two 33 that follow it; from which if the bottom curve towards the right hand (as it was often made formerly) was taken off, the upper part would make the 2. Which agreement between thoſe figures is not only uſual at preſent, but often found in manuſcripts of the fourteenth and fifteenth centuries. Tho' ſometimes indeed it is otherways; and the 2 has an angle at the top, when the 3 is round, which would not ſo well have ſuited this ſquare hand. The reaſon which occaſioned the carrying this date ſo high muſt he preſumes, have been the ſimilitude between the ſmall *i* over the preceeding abbreviated word *Domini* and this 2. But tho' they appear to have ſome likeneſs, yet there is a manifeſt difference between them: For, the 2 is much larger at the top, where it has an angle, and a curve downward, that plainly diſtinguiſh it from the former. Could it be taken for a 1, he ſhould much rather ſuppoſe it was

deſigned

deſigned for a letter than a figure; and the 2 following characters for a double *ll*; and ſo the whole to be only an abbreviation of the word *milleſimo*. But as he thinks it muſt be a 2, for the reaſons already aſſigned, and as he does not remember to have ever met with ſuch a double *ll*, he cannot but eſteem the other the true reading. And yet ſtill he believes this date may claim the preference of being the oldeſt of the ſort, that has hitherto been diſcover'd.

The antiquity, aſcribed to the *Colcheſter* date, namely 1090, has, it ſeems, been occaſioned by a miſtake in the copy: For, the 0 in the place of hundreds ſhould have been made a 4, by drawing down an oblique ſtroke on each ſide from the bottom, which makes it 1490, before which time the 4 had long receiv'd that ſhape, as may be ſeen in Fig. 2. This information Mr. *Ward* had from Mr. *Weſt*, a perſon well ſkilled in the *Britiſh* antiquities, and who himſelf perceiv'd the miſtake in viewing the original.

As to the date from *Widgel-hall*, which gave occaſion to this enquiry, it ſeems to Mr. *Ward* plainly intended to expreſs the year 1000, and no more, by the *Roman* character (Fig. 3.) in the eſcutcheon on the right ſide. For, the characters in the other eſcutcheon cannot, he thinks, ſtand for figures, but muſt be the initial letters of two names J. G. as W. R. in the *Helmdon* date; and were very probably deſigned in both to denote the perſons who erected thoſe buildings. The omiſſion of a character in the place of hundreds is ſtill an argument with him, that theſe two laſt were not made for figures. But what he imagines puts the matter paſt all doubt, is the want of evidence that the figure 6 had receiv'd that form till ſome ages after: And when it was introduced, the upper part was not at firſt made ſo erect, as it is here, but carried in a ſmall arch juſt over the top of the circle, as may be ſeen in Fig. 2. On the other hand what looks here like the modern 6 was at that time the uſual form of the capital G. This he found fully confirmed by a large collection of original grants, made by our ancient kings and others, and preſerv'd in the *Cottonian* library, *Auguſtus* II. Upon conſulting theſe for half a century at leaſt, both before and after the year 1016, Mr. *Ward* found the G ſo written in a great number of them, of which the following are ſome few inſtances. N. 37. *Anno* DCCCCLX. N. 35. *Anno* DCCCCXCIII. N. 53. *Anno* MXLV. N. 49. *Anno* MLXXXI.

For

For these reasons, therefore, he makes no question, but that character was designed for a G and not a 6. And it is plain from other circumstances in Mr. *Gulston*'s letter, that the building might very probably be as ancient as the year 1000; which renders this relique of it, considering how firm and sound it still is, a remarkable curiosity.

The use, Mr. *Ward* thinks may be made of these observations, is this: That so far as hitherto appears, any coin, inscription or manuscript, with a suppos'd date before the thirteenth century, expressed in *Arabian* figures, may be justly suspected either not to be genuine, or not truly read; unless the antiquity of it be certain from other clear and undoubted circumstances, and the date bear no other reading; and if it be a copy, that it has been taken with exactness.

Some Considerations on the Antiquity and Use of the Indian Characters *or* Figures; *by Mr.* John Cope. Phil. Trans. N° 439. p. 131.

THE ingenious invention of figures by the sagacious *Indians* is of such vast importance in numbering, that it can never be sufficiently admired, tho' now a days the use of them is become so familiar among us, that very few consider what a loss the want of them would be to people of every degree and station in life: For, only to consider, that such a number as not long before the conquest would take up a good arithmetician whole days to count by the literal characters, is now by the help of figures commonly express'd by a child in a few minutes. This consideration of the vast use of figures put the learned Dr. *Wallis*, and others after him, upon enquiring at what time they were first happily introduced into this island.

Dr. *Wallis* informs us, that we had the figures from *Spain* whither they were brought by the *Moors*; the *Moors* had them from the *Arabians*; and the *Arabians* from the *Indians*. And it was the Dr's opinion, that they were first brought into *England* about the year 1130: In regard the first instance of their use, which he had met with, was a date upon a chimney piece, which date was 1133 (as represented in Fig. 4.) the character, which the *Romans* made use of to express 1000, being mixed with figures, as he observes, was often done at their first coming in; since that, in *Phil. Trans.* N° 266 is mention'd a date, 1090, all in figures. Mr. *Cope* himself had produced a date upon a chimney-piece at *Widgel-hall* in *Herefordshire*,

which was (as ſhewn Fig. 5.) 1016, the character for the 1000 being here again mixed with figures. And now he produces a ſtill earlier inſtance of the uſe of figures in *England*, as in Fig. 6. which is a draught of an inſcription over a gate-way at *Worceſter*, built, as it is believed, in the reign of king *Edgar*, and is 975, which is 158 years before Dr. *Wallis*'s date; 41 years before that Mr. *Cope* had produced formerly, and is now 760 years ſtanding. The ſhape of the figures in his date were alter'd from what they are here ſhewn to be, about 2 years before, when the gate was new chipped and beautified; and at the ſame time the modern figures 975 were then painted in their room, as they are now to be ſeen; the ground is gold, and the figures black. The account of this date Mr. *Cope* had from Mr. *Joſeph Dougharty* of *Worceſter*, who lived in the houſe over the gate-way on which this inſcription is: He likewiſe informed him, that his houſe goes by the name of *the oldeſt houſe in five counties*: And it is the current opinion thereabouts, and reported by the ancient people in that place, that the houſe was built by king *Edgar*, wherein they ſay - - - he ſometimes kept his court. Mr. *Cope* owns himſelf not ſo well acquainted with the hiſtory of thoſe times, as to ſay whether king *Edgar* either built, or kept his court there; but all hiſtorians agree that *Worceſter* was then a very conſiderable Biſhopric; and that *Dunſtan* and *Oſwald*, who were ſucceſſively Biſhops there in *Edgar*'s time, were both his great favourites; eſpecially *Dunſtan*: For, it appears that the firſt thing *Edgar* did after he came to the crown was to recall *Dunſtan* from *Flanders*, where he had been 3 years in exile; and immediately thereupon was made prime Miniſter, favourite and confeſſor, at firſt Biſhop of *Worceſter*, and afterwards Archbiſhop of *Canterbury*; upon which laſt promotion his great friend *Oſwald* ſucceeded him in the ſee of *Worceſter*: And it is very likely that either *Dunſtan* or *Oſwald*, as having ſo much power, intereſt, and riches, might erect a building there, of which this gate-way might have been a part: For, as *Edgar* died in the ſame year 975; if we ſuppoſe the date to be fixed upon the building the year it was finiſh'd, as is now commonly done, *Edgar* could not live or keep his court there, unleſs it was in ſome part of that year, in which we ſuppoſe it to be finiſh'd.

Mr. *Cope* next mentions ſome obſervations upon the different ſhape the figures have been alter'd to, ſince their coming into theſe weſtern parts: For, our anceſtors wrote them different

from

from the *Indians*; and we again make ſome of them different from what our anceſtors did, as will appear by the table repreſented in Fig. 7.

In this table the left hand column contains the *Indian* characters; the middle thoſe uſed by our forefathers, as appears by old weſtern manuſcripts; the third, the characters we now uſe.

We may now obſerve, that the figure 1 is the ſame as the *Indian*; the figures 2 and 3 the ſame with the *Indian*, only placed in a different poſition, for the ſake of writing them more readily; for, only the daſh from the *Indian* 3 is taken away; they are only, as we may ſay, both ſet upright: So the *Indian* character for four is much the ſame with ours; only we cloſe the head, and ſet it upright, thus 4. Again, our anceſtors transferr'd the *Indian* character for 5 from the place of 5 to that of 8, and with very little alteration is our 8 made from it. As the figure 5 was mov'd into the place of 8; ſo the old eight was mov'd into the place of ſeven: And as they put the five for an eight, they put the ſix into the place of five, which character for ſix was at length alter'd as in the figure, and laſt of all to 5. The 2 characters 9 and 0 are without any alteration, only that our anceſtors ſtruck a line croſs the cypher, which we now leave out; and by that means it is reſtor'd to its ancient form. And now we have no figure left but the *Indian* character for 7 to derive the modern 6 from, to which it ſeems to have no manner of relation: Mr. *Cope* only obſerves, that it ſeems not unlikely to be compounded of the *Indian* 0 (5) and the 1, as thus, 6: For, of the 2 ancient characters for 5, the 0 is *Indian*, and the other *Arabian*; this laſt being no other than the *Arabian* letter (repreſented in Fig. 8.) inverted, which in the *Arabian* alphabet denotes the ſame number; and is, as it is ſuppos'd, us'd by the *Arabians* only.

The *Roman* characters have likewiſe undergone alterations: For, it is found that 1000 was repreſented by the ancients by the character (Fig. 9.) as alſo by the character (Fig. 10.) whence is deriv'd the modern M for that number: Alſo 5000 was repreſented by the character (Fig. 11.) and 50000 by the character (Fig. 12.) and hence the modern characters IↃ and IↃↃ for the ſame number. We alſo find in ancient inſcriptions the characters (Fig. 13) ſtand for 20, and that in Fig. 14 for 30; the letter X being twice expreſs'd in the one, and three times in the other, which the moderns write ſingle, as XX and XXX, only the timber-merchants uſe the ancient characters (as repreſented in Fig. 13 and 14.) to this day.

Remarks on the aforesaid ancient Date, *over a Gate-way near the Cathedral at Worcester; by Mr.* John Ward. Phil. Trans. N° 439. p. 136.

HAVING in a former *Transaction* communicated to the *Royal Society* some remarks on an ancient date, carv'd in wood, that was found at *Widgel-hall* near *Buntingford* in *Hertfordshire*, with the characters (represented Fig. 3. Plate VI.) 16, which had been read 1016, supposed to be mixed numbers, the character (Fig. 3.) being *Roman*, and the 2 others *Arabian* or *Indian*, as they are indifferently call'd: This led Mr. *Ward* to consider two other dates of the like kind, formerly publish'd in the *Phil. Trans.* one found at *Helmdon* in *Northamptonshire*, in mixed characters (Fig. 3.) expressing, as was thought 1133; and the other at *Colchester*, said to denote the year 1090, entirely in *Arabian* figures. But upon searching into the origin of those figures, and the time when they were first brought into these parts of the world, he could meet with no examples of them in any manuscripts, before some copies of *Johannes de Sacro Bosco* (mention'd by Dr. *Wallis*) who died *An.* 1256, which was 123 years after the latest of the 3 dates above-mention'd. As it could not, therefore, but seem very surprising, that workmen should have made use of those figures for such common purposes, so long before they appear in the writings of the learned: So upon a closer examination and farther enquiry, he found there was no reason from any of these dates to suppose, it was really true in fact: For, the *Helmdon* date instead of 1133 (Fig. 3.) should, as he then shew'd, be read 1233; the *Colchester* date 1490, instead of 1090: And that at *Widgel hall* has no *Arabian* figures in it, the characters 1 and 6 not being numbers but the initial letters of two proper names I G, in the usual form of those letters in that age.

But there was read before the society an account of a date at *Worcester* (Fig. 6.) more ancient than any of the three former; in which the unit is a *Roman* numeral, and the other two taken for *Indian* figures. He observ'd above, that such mixtures were sometimes found in ancient numbers; tho' in what manner they were so used, he did not then explain, but for brevity contented himself with referring to the *Algebra* of Dr. *Willis*, a book so very well known. The Dr. thought it necessary to take notice of this, in order to account for his way of reading the *Helmdon* date, in which the character (Fig. 3.) only is a *Roman* numeral: And Mr. *Ward* himself had

had met with a few instances of it in Dr. *Mead*'s manuscript of *Boethius*, *De Arith. lib.* 2. as CCC29, and DCC68, where the hundreds are numeral letters, and both the decimals and units *Arabian* figures, but it is observable, this is not done promiscously, but the largest numbers are always letters, and the lesser ones, figures, as in the *Helmdon* date. And *Mabillon De re diplom. tab. xv.* has observ'd, that in a curious manuscript copy of *Thomas a Kempis*, written in the 15th century, some of the pages are so number'd: Which method, so far as appears, was always attended to, and never in any one instance inverted. So that this *Worcester* date, which has a *Roman* numeral in the place of units, and the two preceding characters supposed to be *Indian* figures, is not only without example, but directly contrary to all other instances of such mixed numbers; which consideration alone might be a sufficient ground to think, there must be some mistake in the reading.

But the middle figure, taken for a seven, is as remarkable; which, turning towards the left hand, forms two obtuse angles; one above, the other below. This shape of the seven, he believes, was never seen before, and seems by no means to suit that age. In the specimen of the figures, taken from *Johannes de Sacro Bosco* by Dr. *Wallis*, as may be seen in Fig. 2. the figure seven is made in the form represented in (fig. 15.) like the two legs of an isosceles triangle. And in *Roger Bacon*'s Calendar, dated 1292, there is only this variation; that the leg to the left hand is somewhat shortned, as will likewise appear in the Table fig. 2. And this form continued till printing was introduced among us; as is evident from *Caxton*'s *Polychronicon*, and other books printed about that time: Nor does Mr. *Ward* find it till more latterly in any other shape, except that in Bishop *Beveridge*'s table of *Indian* figures, *Arith. chron. lib.* 1. *c.* 4. the two legs of our ancient seven are drawn parellel, and arch'd at top (as in fig. 16.) instead of meeting in an angle: And *Planudes*, a *Greek* writer, has kept the true *Arabian* form (V) like the *Roman* five, which the *Europeans* inverted, as in fig. 2. The last alteration this figure receiv'd among us, was by raising the shorter leg horizontally: But no instance of it parallel to this in the *Worcester* date, nor any thing like it, has before appear'd: As there seems therefore no reason to suppose it a seven, so Mr. *Ward* thinks it was probably design'd for the *Roman* numeral, ten, which was made in this form, like an X; to which character in the old square hand, this supposed seven (fig. 17.) would

very

very well agree, by ſupplying only the two extreme parts to the right hand, as in fig. 18. which may be eaſily ſuppoſed to have been decay'd by length of time.

As there is no reaſon to take the middle character for a ſeven, ſo neither is there any to ſuppoſe the firſt intended for a nine, being thus placed before two *Roman* numerals, as Mr. *Ward* takes them both to be: It has indeed ſome ſimilitude to that figure; but nothing more than what anciently was, and ſtill is, common to the letter (fig. 19.) in that hand, which reſembles a double character (fig. 20.) with an oblique ſtroke turned inwards from the bottom of that to the right hand; ſo that if the other to the left be taken away, what remains will appear in the form (fig. 21.) like what is here call'd a nine: And every one, who has any acquaintance with ancient inſcriptions knows that letters frequently periſh in this manner, one part before another.

Upon theſe ſuppoſitions the true reading would be MXV: But ſince the old date is now deſtroy'd, and modern figures put in its place, this muſt remain uncertain. And Mr. *Ward* cannot but think the former characters muſt have been very dark and obſcure, for the following reaſons.

There is a tower, as he is inform'd, over this gate, of which Dr. *William Thomas*, who lives very near it, has given ſome account, in a treatiſe, entitled, *A Survey of the Cathedral Church of Worceſter*, p. 7. 'it is commonly call'd King 'John's tower, and ſaid by ſome to be built by him: But it 'was much more ancient; having in the front of it the ſtatues 'of King *Edgar*, and his two Queens *Ethelfleda* and *Ethelfrida*; and the ſtreet, it leads into, in ſeveral writings is 'call'd *Edgar-ſtreet*.' See likewiſe *Hearne*'s preface to *Heming*'s *Chartularium*, in the frontiſpiece of which treatiſe is a draught of thoſe 3 figures. Could there be any room for its being ever ſuppoſed to have been built by King *John*, while this date was plain and clear? Or would the author of the ſurvey have contented himſelf with only ſaying, *it was much more ancient*; when he could ſo eaſily have given us the year, had he been ſatisfied with the reading. King *Edgar* had been a great benefactor to the Cathedral Church at *Worceſter*; and is ſaid to have given it 300 hides of land, vide Dr. *Thomas*'s *account of the Biſhops of* Worceſter. *Edgar* died in 975, but his Queen *Ethelfrida* ſurvived him ſeveral years. And as it is not unuſual, in order to perpetuate the memory of publick benefactors, to erect ſtatues and other monuments of them, after

they

they are dead, it might be ſo in the preſent caſe, and the ſtreet receive its name (for ſome time at leaſt) from this building. But tho' the preciſe year of this date cannot now be determined with certainty: It is ſufficient to have ſhewn, that neither the order of the characters, their ſhape, nor the oldeſt examples of *Arabian* or *Indian* figures, any where found, do in the leaſt countenance the reading given it: But on the contrary, all of them afford the higheſt probability, that it cannot be genuine.

Mr. *Ward* only adds, that Mr. *Gale* and Mr. *Weſt* told him, they thought the two firſt characters taken for a nine and a ſeven, might probably have been nothing but the character repreſented in Fig. 19. which will bring the date to 1005, ten years nearer the time of *Edgar*. Mr. *Ward*'s only difficulty as to that reading is, that the character would then have two oblique ſtrokes (Fig. 22.) prolonged from the bottom, one in the middle, beſides the other uſual one towards the right hand, which Mr. *Ward* does not remember ever to have met with: But as this inaccuracy might ariſe from the obſcurity of the character; he leaves it to the curious to judge either way as they pleaſe, both ſentiments equally ſuppoſing the original characters of this date muſt have been *Roman* numerals.

This enquiry is the more neceſſary, as it is apprehended to be a matter of ſome conſequence, eſpecially with relation to manuſcripts: A copy, for inſtance, of ſome ancient author, written in 1375, and dated in *Arabian* figures, may, by changing only the 3 into a cypher, be carried back 300 years; or by making it a nine, and taking out the 1, may be raiſed ſtill a century higher, to 975, the ſuppoſed year of the *Worceſter* date: And thoſe converſant in manuſcripts are ſenſible that the age of them cannot always be determined barely by the hand: Since, therefore, *Arabian* figures are in moſt caſes much more eaſily falſified than *Roman* numerals, Mr. *Ward* preſumes a too great caution cannot be uſed, in admitting any inſtances of them more early, than have hitherto been diſcovered, but upon very clear and ſufficient evidence.

Experiments on Electricity; by Mr. Gray. Phil. Tranſ. N° 439. p. 166.

FEB. 18. Mr. *Gray* tried what effect would be produced on ſeveral ſorts of wood, with reſpect to the luminous part of electricity; the wood (which was fir, aſh, and holly) was made into rods of the ſame form with the iron ones mentioned in former

former experiments; theſe being ſucceſſively diſpoſed upon electric bodies, in the ſame manner as the iron rods had been, and the tube applied to one end, there appeared a light on it, but not with ſo great a force, nor did it extend to ſo great a length; neither was its form conical, but rather cylindrical; but its extremity ſeem'd to conſiſt of a ſhort fringe of light; when the light, communicated to the rod by the application of the tube, ceaſed, upon a motion of the hand towards the point of the rod, the light came out again, as was mentioned of the iron rods; but when the hand or finger was held near the point of theſe wooden rods, there was no pricking or puſhing of the finger felt, as when the iron rods were made uſe of. Mr. *Gray* had ſome of theſe rods made much bigger at one end, than the other, and now applying his finger to the larger end, there not only appeared a light, but the finger was puſhed, more eſpecially when the holly rod was made uſe of; and the cheek was a little pricked, but the ſmart was not near ſo great, as when the iron rods were uſed; the great end of the rod was pointed with a much larger angle than the leſſer one; yet there was very little, if any difference, in the form or bigneſs of the light that proceeded from either end.

Having procured two pair of lines of worſted yarn, one a mazarin blue, the other a ſcarlet colour, on the 3d of *April* he ſuſpended the boy, firſt on the blue lines, and he found all the effects the ſame, as when he was ſuſpended on lines of blue ſilk: He then ſuſpended him upon the ſcarlet lines; but now tho' the tube was as well excited, and the experiment often repeated, yet there was no effect produced on him, either of attraction of a pendulous thread, nor of pricking or burning, by applying on hand near him; he then laid one of the iron rods, firſt upon the blue lines and all the ſame effects were exhibited, as when the ſame rod had been laid on ſilk lines of that colour; but upon laying the ſame rod upon the ſcarlet lines, no manner of attraction, *&c.* was perceived.

Mr. *Gray* now found, that when the ſtand with the little ivory cups (mentioned in the experiments on the communicative electricity of water) is ſet upon any electric body, the ſame phenomena are produced, not only by holding the tube near the water, but when that is removed, and the tip of the finger placed over the water, there is a little hill or protuberance of water of a conical form, from the vertex of which proceeded a light, and a ſmall ſnapping.

May

May 6, the following experiment was made: The boy being ſuſpended on the ſilk lines, and the tube applied near his feet, as uſual: upon his holding the end of his finger near a gentleman's hand, that ſtood on a cake made of ſhell-lac and black roſin; and at the ſame time another gentleman ſtanding at the other ſide of the boy with the pendulous thread; the boy was then bid to hold his finger near the firſt gentleman's hand; upon which it was pricked, and the ſnapping noiſe heard; and at the ſame time the thread which by its attraction was going towards the boy, fell back, the boy having loſt a great part of his attraction; upon a ſecond moving his finger to the gentleman's hand, the attraction ceaſed; then the thread being held near that gentleman, he was found to attract very ſtrongly: But having afterwards repeated this experiment, Mr. *Gray* found that tho' the attraction of the boy is much diminiſhed, yet he does not quite loſe it, till 2, 3, and ſometimes 4 applications of his finger to the hand of him, that ſtands on the electric body, but without touching him. At another time he cauſed three perſons to ſtand, one on a cake of ſhell-lac, *&c.* the other upon one of ſulphur, the third on a cake of bees-wax and roſin; the perſons all holding hands, and the boy applying his finger near the firſt man's hand, they all three became electrical, as appeared by the attraction of the thread, when held near any of them.

June 10, in the morning Mr. *Gray* repeated the experiments with the wooden rods, the moſt material of which were made with the holly rod; and this being laid on the glaſs cylinder, and a fir board, about a foot ſquare and $\frac{3}{10}$ of an inch thick, placed erect upon a ſtand, which was ſet on another glaſs cylinder; ſo that the centre of the board was placed near the point of the rod, but not to touch it by near half an inch; then the tube being held near the great end of the rod, there iſſued out a light from the little end of the rod, which was that next the board; and as the boy told Mr. *Gray*, it came along with a hiſſing noiſe, and ſtruck againſt the board: When he touch'd the board, there was a light; and at the ſame time, another on the end of the rod, but he heard no ſnapping nor pricking of his finger, as when the braſs plate and iron rod were made uſe of.

When the boy was ſuſpended upon the ſcarlet lines, he attracted the white thread at a very ſmall diſtance, but the attraction ceaſed in about 6 or 7 ſeconds of time: Then the boy being taken off, an iron rod was laid on the lines;

but there was no attraction of the thread by the body of the rod; but when the thread was held near either of its pointed ends, there was a ſmall repulſion of it, and in the dark a very ſmall light was ſeen at each end of the rod.

When the boy was ſuſpended upon the blue lines, he attracted the thread to him, when it was held at leaſt a foot diſtance from him, and continued his attraction to near 75 ſeconds; the iron rod continued its attraction not more than 36 ſeconds.

In the afternoon when the boy was ſuſpended on the blue lines, he continued his attraction 50 minutes; on the ſcarlet lines, 25 minutes; on the orange coloured lines, 21 minutes.

By theſe experiments we ſee the efficacy of electricity on bodies ſuſpended upon lines of the ſame ſubſtance, but of different colours, and alſo that the attraction continues much longer upon ſilk than yarn; and conſequently ſilk is the propereſt body to ſuſpend bodies upon, to which we would communicate electricity.

An Account of the Births and Burials, with the number of Inhabitants at Stoke-Damerel, *in the County of* Devon; *by Mr.* William Barlow. Phil. Tranſ. N° 439. p. 171.

UPON taking a ſurvey about *Michaelmas* 1733, of the inhabitants of *Stoke-damerel*, the number of perſons (men, women, and children) reſiding in the pariſh, amounted to 3361. By the regiſter Mr. *Barlow* finds, that in the ſame year, 28 couple were married, 61 males, and 61 females baptized and 62 people buried.

Baptized.	Buried.	Number of people
122.	62.	3361.

Whence it appears, that the number of perſons who died, is one more than half the number of children born, and that about 1 in 54 died.

It is to be obſerv'd, that the general fever, which almoſt all the inhabitants of the ſame place were ill of at the ſame time, was in part within the year mentioned; alſo that one of the perſons included in the number of thoſe buried, was a foreigner brought from on board a *Dutch* ſhip in the river; and that two more were drowned from on board a man of war; but the ſhip's company is not included in the number of the inhabitants.

It appears both from experience and obſervation, that the pariſh of *Stoke-damerel* is as healthful an air as any in *England*.

The

The foramen ovale of the Heart found open in an Adult; by Mr. Amyand. Phil. Tranſ. N° 439. p. 172.

MR. —— dying at the age of 22, of an illneſs that had perplexed his phyſicians, was opened, to diſcover an impoſthume, which was apprehended in the belly: As nothing was obſerv'd there worth notice, but a very great relaxation of the *viſcera*, the cauſe of his death was looked for in the *thorax*; and there the lungs were ſtrongly attached on each ſide, to the *pleura*, and a large collection of water in each cavity, eſpecially on the left, where the poſterior lobe was inflam'd and tending to ſuppuration: The quantity of water in the *pericardium* was greater than uſual, and the heart much larger than could be expected in ſo great an atrophy, as the patient was reduced to; in it the *foramen ovale* was found open, ſo as to give paſſage to a large finger, when a fungous ſubſtance, which grew from the circumference of the foramen, and ſtopt up the ſame, was remov'd: The valve was hardly perceptible, being callous and furl'd up: The *ductus arterioſus* was found cloſe as uſual. This patient had enjoy'd great health till latterly, and gave no ſign of this opening of the *foramen ovale*, which is preter-natural in adults.

A Propoſition relating to the combination of tranſparent Lens's *with reflecting planes; by Mr.* Hadley. Phil. Tranſ. N° 440. p. 185.

THE effects of the combining ſeveral kinds of teleſcopes with reflecting planes led Mr. *Hadley* to the following propoſition.

That if two lens's of equal focal length be put together in the form of a teleſcope, and a plane ſpeculum be placed before one of them, ſo that the axis of the teleſcope make any angle with its ſurface, and a ray of light (the line of whoſe direction lies in a plane perpendicular to that ſurface, and paſſing thro' the axis of the teleſcope) fall on it, and be reflected from it, ſo as to paſs thro' the teleſcope; then the line of its laſt direction, after paſſing the teleſcope, will make an angle with that of its firſt direction, before its incidence on the ſpeculum, very nearly equal to double the angle, made between the axis of the teleſcope and the ſurface of the ſpeculum.

Lemma. Let the line F G (fig. 23. pl. VI.) be the common axis of the two lens's I D and K E, of equal focal lengths; to which let the lines A D, D B and B E, be each equal; and let a ray of light, iſſuing from a point in the axis F, fall on the lens I D at I, and be there refracted into the line I G, cutting the axis in G, and meeting the lens K E in K; where let the ray be again refracted into the line K H, cutting the aforeſaid axis in H: The angles I F D and K H E are very nearly equal.

Demonſtration. We learn from dioptrics, that the lines F I, I G, K H, and F G are all in the ſame plane; and by the conſtruction, the lines A D, D B and B E are equal; and by *propoſ.* 20. of *Huygens*'s dioptrics, the lines F A, F D and F G are continually proportional; and conſequently F A is to A D, as F D to D G; and by diviſion, F A is to A D, as F D — F A (= A D) is to D G — A D (= B G.) Therefore, A D is to B G, as F D to D G. By the ſame propoſition, the lines B G, E G and H G are alſo continually proportional, and B E (= A D) is to B G, as E H is to E G. Hence it follows, that the lines F D, D G, and E H, E G are proportionals: But as F D is to D G, ſo is the tangent of the angle I G D, or K G E to the tangent of the angle I F D; and as E H is to E G, ſo is the tangent of the angle K G E to the tangent of the angle K H E: The tangent of the angle K G E, therefore, has the ſame proportion to the tangents of each of the angles I F D and K H E, and conſequently thoſe angles are equal Q. E. D.

N. B. In the demonſtration of the above cited propoſition of *Huygens*, the thickneſs of the lens's is neglected, and the diſtance of the points I and K, from the line F G, ſuppoſed very ſmall; ſo that if either of thoſe are too great, there may ariſe a ſenſible difference between the angles I F D and K H E.

Let D F and C G (fig. 24.) repreſent the two lens's put together as before, having their common axis in the line E L, and B N a plane ſpeculum, to which that line is inclined in the angle G H N, and let A B be a ray of light, falling on the ſpeculum at B, as is before expreſſed, and let it be there reflected towards the point C of the lens C G, where it is refracted towards the point D of the lens D F, and there again refracted into the line D E, cutting the axis in E: The angle A O P contained between this laſt line D E, continued backwards, and the firſt line of the incidence of the ray A B, will be very nearly equal to double the angle

of

of inclination of the axis of the lens's EL to the plane of the ſpeculum BN; *i. e.* double the angle GHN.

Demonſtration. Produce the lines of incidence and reflection of the ray AB and BC, till they meet the axis of the two lens's in I and L; and thro' the point B draw BK perpendicular to the plane of the ſpeculum, and cutting the ſame axis in K, the angles KBL and KBI are equal: The angle KLB is the difference of the angles IKB and KBL; and the angle HIB is the ſum of the angles IKB and KBI (equal to KBL): Therefore, the angle IKB is equal to half the ſum of the angles HIB and KLB: But by the foregoing *lemma*, the angles KLB and FED are very nearly equal: Therefore, the angle IKB is nearly equal to half the ſum of the angles HIB and FED: *i. e.* to half the angle POB; and its complement BHI or GHN is nearly equal to half the angle AOP, the complement of POB to a ſemi-circle. Q. E. D.

If the firſt incidence of the ray be ſuppoſed to be in the line ED, it will proceed in the ſame track as before, but with the contrary directions; ſo that the angle EOB made between the firſt incident ray, and the laſt reflected one, will ſtill be equal to the double of GHN, as before.

It is evident that on this principle an inſtrument might be conſtructed, whoſe effects would, in a great meaſure, reſemble thoſe of that in *Phil. Tranſ.* N° 420: But it would be liable to the errors ariſing both from the ſpherical figure of the lens's, and the different refrangibility of the rays of light, when the object is ſeen at a diſtance from the axis of the teleſcope; altho' thoſe errors, by a proper diſpoſition of the parts of the inſtrument, may be reduced to a very ſmall quantity: However, for this reaſon, and becauſe the inſtrument ſeemed to Mr. *Hadley* to be attended with greater inconveniencies, both in its conſtruction and uſe, than the other, he has not thought it neceſſary to give a more particular deſcription of it.

A large bony ſubſtance found in the Womb; *by Dr.* Hody. Phil. Tranſ. N° 440. p. 189.

IN examining the pelvis of a woman, 57 years of age, Dr. *Hody* found a large bony ſubſtance in the womb, and ſo ſtrictly united to it, that they ſeem'd one and the ſame body: Upon cutting the ſubſtance aſunder, he obſerved the oſſification went no farther than the thickneſs of a ſhilling; the part

part immediately under the oſſification was like firm fleſh, and this fleſh grew ſofter and ſofter, as it drew near its centre.

This woman never had but one child, of which ſhe was delivered about 27 years before ſhe died: Her chief complaints for ſome years, were a ſhort cough, great difficulty in breathing, frequent uneaſineſs in making water, or in going to ſtool, and a conſtant weight, or bearing down, upon the parts of generation.

The immediate cauſe of her death was, undoubtedly, an *aſthma*; for, ſhe had only one lobe of the lungs left, that was perfectly ſound; the reſt adhered firmly to the *pleura*, were very much contracted, and in ſome places ſcirrhous.

A (Fig. 1. Plate VII.) repreſents the bony ſubſtance; B the ſubſtance of the womb cut open, and turned backwards; C ſmall fibres connecting the bony ſubſtance with the *uterus*; D the right *Fallopian* tube lying upon the membrane, which joins the tube to the *uterus*, and to the *ovarium*; E the *ovarium*; F the *Morſus diaboli*; G the left *Fallopian* tube cut off; H the neck of the *uterus* cut open, as elongated by the diſeaſe; I the mouth of the *uterus* laid open; K the greateſt part of the *vagina* likewiſe laid open.

This *uterus*, with the bony ſubſtance adhering thereto, having been kept 10 years in ſpirits before it was ſent to the engraver, the *vagina*, *Fallopian* tube, the membrane on which the tube lies and the *ovarium*, muſt be ſuppoſed to be greatly contracted: But that part of the *uterus* diſtended by the bony ſubſtance is indeed very little contracted: For, when firſt taken out of the body it was ſcarce as thick as a half crown piece.

Some Experiments concerning the impregnation of the Seeds of Plants; *by Mr.* Logan. Phil. Tranſ. N° 440. p. 192.

AS the notion of a male ſeed, or the *farina fœcundans* in vegetables is now very common, Mr. *Logan* only mentions ſuch obſervations, as may have ſome tendency to the ſubject in hand: And firſt, he finds from Mr. *Miller*'s dictionary, that M. *Geoffroy*, from the experiments he made on *Maiz*, was of opinion that ſeeds may grow up to their full ſize, and appear perfect to the eye, without being impregnated by the *farina*; which poſſibly, for ought Mr. *Logan* knows, may in ſome caſes be true, as there is no end of varieties in nature. But in the ſubject M. *Geoffroy* mention'd, Mr. *Logan* has reaſon to believe it is otherwiſe; and that he did not apply all the care requiſite in the management.

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In ſpring Mr. *Logan* reſolv'd to make ſome experiments on maiz or *Indian* corn. In each corner of his garden (which is 40 foot in breadth, and near 80 foot in length) he planted a hill of that corn; and watching the plants when they grew up to a proper height, and were puſhing out both the taſſels above, and ears below: From one of thoſe hills he cut off the whole taſſels; on others he carefully open'd the ends of the ears; and from ſome of them he cut or pinch'd off all the ſilken filaments; from others he took about half, from others one fourth, and three fourths, *&c.* with ſome variety, noting the heads, and the quantity taken from each: Other heads again he tied up at their ends (juſt before the ſilk was putting out) with fine muſlin, but the fuzzieſt or moſt nappy he could find, to prevent the paſſage of the *farina*; but that would obſtruct neither ſun, air, or rain. He alſo faſten'd it ſo very looſely, as not to give the leaſt check to vegetation.

The conſequence of all which was this; that of the 5 or 6 ears on the firſt hill, from which he had taken all the taſſels, from whence proceeds the *farina*, there was only one that had ſo much as a ſingle grain in it; and that in about 480 cells had but about 20 or 21 grains; the heads or ears, as they ſtood on the plant, look'd as well to the eye as any other: They were of their proper length; the cores of their full ſize: But to the touch, for want of the grain, they felt light and yielding. On the core, when diveſted of the leaves that cover it, the beds of ſeed were in their ranges, with only a dry ſkin on each.

In the ears of the other hills, from which he had taken all the ſilk; and in thoſe he had cover'd with muſlin, there was not ſo much as one mature grown grain, nor other than what he has mention'd in the firſt: But in all the other ears, in which he had left part, and taken part of the ſilk, there was in each the exact proportion of full grains, according to the quantity or number of the filaments he had left on them. And for the few grains he found on one head in the firſt hill, he immediately accounted thus: That head, or ear, was very large, and ſtood prominent from the plant; pointing weſtward with its ſilk directly towards the next hill of *Indian* corn; and the *farina*, he knows, when very ripe, on ſhaking the ſtalk, will fly off in the fineſt duſt, ſomewhat like ſmoke. He therefore, with good reaſon, judged that a weſterly wind had waſted ſome few of theſe particles from the other hill, which had lit on the ſtiles of this ear, in a ſituation perfectly well fitted to receive them, which none of the other ears on the ſame hill had: And indeed

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he was ſurpriſed that he found no more of the ſame ear impregnated in the ſame manner.

As Mr. *Logan* was very exact in this experiment and curious enough in his obſervations, he thinks it may reaſonably be allow'd, that notwithſtanding what M. *Geoffroy* may have deliver'd of his trials on the ſame plant, that the ſilk was taken quite away from thoſe heads, and that in ſuch as were cover'd with muſlin none of the grains will grow up to their ſize, when prevented from receiving the *farina* to impregnate them; but appear, when the ears of corn are diſcloſed, with all the beds of ſeeds or grains in their ranges, with only a dry ſkin on each, about the ſame ſize as when the little tender ears appear fill'd with milky juice before it puts out its ſilk. But the few grains that were grown on the ſingle ear were as full and as fair as any he had ſeen; the places of all the reſt had only dry empty pellicles as he has deſcribed them; and he much queſtions, whether the ſame does not hold generally in the whole courſe of vegetation, tho' it may not be ſafe to pronounce abſolutely upon it, without a great variety of experiments on different ſubjects. But he believes there are few plants that will afford ſo fine an opportunity for obſervation as the maiz or *Indian* corn; becauſe its ſtiles may be taken off or left on the ear, in any proportion; and the grains be afterwards number'd in the manner mention'd.

Some Obſervations of Eclipſes *of* Jupiter's Satellites, *at* Southwick, *near* Oundle *in* Northamptonſhire; *by Mr.* Lynn. Phil. Tranſ. N° 440. p. 196.

THE teleſcope Mr. *Lynn* made uſe of in the following obſervations is the ſame as formerly, having a 13 foot object glaſs, with an aperture of 2 inches and 4 tenths, and an eye-glaſs of 2 inches and ½ *focus*; by apparent time at *Southwick*, near *Oundle* in *Northamptonſhire*; longitude weſt from *London* 30 minutes as follows.

Year		Mon.	D.	H.	M.	S.
1730,	The ſecond ſatellite began to emerge	*Apr.*	29	10	19	20
	In a minute after it ſeemed to be as bright as the 4th ſatellite				20	20
	And at full brightneſs about				21	20
1729-30	The 4th ſatellite emerged	*Jan.*	25	9	45	near.

Year

Year		Mon.	D.	H.	M.	S.
1730	The ſecond ſat. began to immerge	*Nov.*	28	13	17	46
	And was quite out of ſight				19	46
1731	The ſecond ſat. began to emerge	*Mar.*	29	11	33	8
	And ſeemed at full brightneſs about				36	30
	The firſt ſat. began to emerge	*Apr.*	18	11	45	10
	And was at full brightneſs about				46	10
	Again the firſt began to emerge	*May*	4	10	4	30
	Was at full brightneſs about				5	45
	And paſs'd by the third ſatellite at			11	49	30
1730-1	The firſt ſat. began to immerge	*Jan.*	7	12	2	55
	And was quite out of ſight about				4	25
1731-2	The third ſat. began to immerge	*Jan.*	28	14	16	00
	Was but equal in light to the ſecond at				19	00
	Quite diſappear'd at				24	00
1732	The ſecond ſat. began to emerge	*Apr.*	30	12	23	57
	The firſt ſat. began to emerge	*May*	6	12	48	44
1732-3	The third ſat. quite diſappear'd	*Feb.*	18	13	5	30
1733	Again the third ſat. diſappear'd	*Apr.*	2	13	3	30
	But it began to fail of its light about 5 or 6 minutes before					
1735	The ſecond ſatellite immerged	*May*	28	10	45	00
	The third began to emerge	*Aug.*	3	9	10	30
	And was 4 or 5 minutes before it came to its full brightneſs.					

Concerning the High Tide *in the River of* Thames *on* Feb. 16, 1735-6; *by Mr.* Jones. Phil. Tranſ. N° 440. p. 198.

MR. *Jones* having in a former *Tranſaction* given an account of the tides flowing in the river of *Thames*, which then flow'd 20 foot 5 inches and a half, as he took it by a level from that high water mark to low water the next morning, and was 4 inches higher than had been known for 40 years before: He now adds, that having marked that high tide on a poſt; on the 16. of *February*, 1735-6 the tide roſe at the ſame place 6 inches $\frac{3}{4}$ above that mark, and flow'd near 2 foot the laſt half hour but one before high water.

Had the tide flow'd its full time, it would have flow'd half an hour longer, and drowned the whole level.

A ſingular cutaneous Diſtemper; *by Dr.* Abraham Vater. Phil. Tranſ. N° 440. p. 199. *Tranſlated from the* Latin.

IN *Phil. Tranſ.* N° 424 is an account of a very extraordinary and curious caſe of a diſtempered *cutis* or rather *cuticula* in a country lad of 14 years of age; it was ſo thick and hard as to be rather like the bark of a tree than a human ſkin; and like a caſe it covered his whole body; excepting the palms of the hands and ſoles of the feet, that he ſeemed to be enveloped in it; it was yearly ſhed in autumn.

This caſe is very well illuſtrated by the hiſtory of a girl of 8 years of age, at *Gera* in *Voigtland*, a ſeat of the counts of *Reuſſen*, as the Dr. had it from Dr. *Harniſchius*.

For ſome years before, ſhe had laboured under a ſwelling of her joints and whole body; and after the uſe of ſeveral quack and other medicines, a remarkable hard tumour aroſe on her back between the ſhoulder-blades, which being taken for a beginning bunch, it was diſcuſſed by topical applications. But from that time there inſenſibly began to grow on her hands and feet, eſpecially on the palms and ſoles, a dry hard cruſt, that projected a good way on the ends of the fingers and toes, and hindered her graſping and walking, ſo that ſhe could neither ſtand nor go, but was fain to be carried. At times the cruſt ſheds, eſpecially after various inunctions; but thereby ſhe became ill, and ſwoln, with anxiety and pains internally, that continued till the cruſt was again renewed: Upon which ſhe felt no other inconveniency then being depriv'd of the uſe of her hands and feet. In a year after, upon the diſorder recurring ſhe was committed to the care of a ſurgeon, who with mercurial laxatives and cleanſing decoctions removed the diſeaſe, and reſtored her ſkin to its former cleanneſs: So that ſhe ſeemed to enjoy a perfect ſtate of health; but whether it ſhould continue time muſt ſhew. Upon viewing this cruſt with a microſcope, it plainly appears to conſiſt of little ſcales, which the more evidently ſhews, that it is nothing other than the cuticle itſelf, expanded and harden'd by a viſcid and tartarous nutriment. The ſame Dr. *Harniſchius* alſo relates that in a neighbouring town there is a young woman, who twice a year for a long time ſhed ſuch a cruſt from her hands and feet as alſo from the arm, and found no relief from any medicines hitherto made uſe of: But this latter patient laboured under an obſtruction of the *Menſes*, which ſeemed to be the ſource of this diſorder.

Expe-

Experiments concerning the Vibrations of Pendulums; by Dr. Derham. Phil. Tranſ. N° 440. p. 201.

THE account Mr. *Bradley* gave in *Phil. Tranſ.* N°432, of obſervations made at *Jamaica* by Mr. *Campbell*, with a nice pendulum clock of Mr. *Graham*'s making, brought to Dr. *Derham*'s mind ſome experiments he made ſome years before, that may be of uſe in obſervations of this nature.

The firſt he takes notice of are ſome experiments he made in 1704, with excellent inſtruments, concerning the vibrations of pendulums in *vacuo*, that were publiſhed in *Phil. Tranſ.* N° 294: The ſum of which is that the vibrations in *vacuo* were larger than in the open air, or unexhauſted receiver; as alſo, that the enlargement or diminution of the vibrations, was conſtantly in proportion to the quantity of air, (or the rarity or denſity thereof,) which was left in the receiver of the air-pump. And as the vibrations were larger or ſhorter, ſo the times were augmented or diminiſhed accordingly, *viz.* two ſeconds in an hour ſlower, when the vibrations were largeſt, and leſs and leſs, as the air was re-admitted, and the vibrations ſhortned.

But notwithſtanding the times were ſlower, as the vibrations were larger, yet he had reaſon to conclude, that the pendulum really mov'd quicker in vacuo, than in the air; becauſe the ſame difference or enlargement of the vibrations (as two tenths of an inch on a ſide) would cauſe the movement inſtead of two ſeconds in an hour, to go ſix or ſeven ſeconds ſlower in the ſame time, as he found by nice experiments.

The next experiments the Dr. mentions, he made at ſeveral times, *viz.* in 1705, 1706, and 1712, by the help of a good month-piece that ſwings ſeconds: The weight that then drove it was about 12 or 13 pounds, and it kept time exactly by the ſun's mean motion: But by hanging on ſix pounds more, the vibrations were enlarged; but yet the clock gain'd 13 or 14 ſeconds in a day.

And as the increaſe or diminution of the power that drives the clock, accelerates or retards its motion; ſo, no doubt, doth cleanneſs or foulneſs affect it; and ſo doth heat and cold: For, all have the ſame effect upon the pallets and pendulum.

The laſt experiments the Doctor mentions, he made in 1716, and 1718, to try what effects heat and cold had upon

iron rods of the ſame length, or as near as poſſible to thoſe that ſwing ſeconds. He made his experiments with round rods of about a quarter of an inch in diameter, and with ſquare rods, about $\frac{3}{4}$ of an inch ſquare; the effects on both which were the ſame.

At firſt he took the exact length of the rods in their natural temper: Then he heated them from end to end in a ſmith's fire, nearly to a flaming heat; by which means they were lengthened two tenths of an inch. Then he quench'd them in cold water, which made them $\frac{1}{100}$ of an inch ſhorter than in their natural ſtate.

Then he warmed them, as near as he could gueſs, to the temper of his body, by which means they were about $\frac{1}{1000}$ of an inch longer than in their natural temper.

Afterwards he cool'd them in a ſtrong frigorific mixture of common ſalt and ſnow, which ſhortened them $\frac{3}{1000}$ parts of an inch.

Afterwards he meaſur'd theſe rods, when heated in an hot ſun, which lengthen'd them $\frac{2}{1000}$ parts of an inch more than their natural temper.

All theſe experiments ſeem to concur in reſolving the phenomenon of pendulum clocks going ſlower under the equator, than in the latitudes from it: But yet he owns, that he has too good an opinion of Sir *Iſaac Newton*'s notion of the ſpheroidal figure of the earth, to part eaſily with it; and therefore he leaves it to the conſideration of others, how far the figure of the earth, and how far heat and cold, and the rarity and denſity of the air, are concerned in that phenomenon.

The Conſtruction and uſe of ſpherical Maps, or ſuch as are delineated upon Portions of a ſpherical Surface; *by Mr.* John Colſon. Phil. Tranſ. N° 440. p. 204.

GEographical maps and hydrographical charts, tho' repreſentations of a convex ſperical ſurface; yet at firſt were delineated upon planes, as being the moſt eaſy and obvious, tho' not the moſt natural and accurate repreſentations: And they will be ſufficiently near the truth, when the part of the earth or ſeas to be deſcribed is not of a very large extent; and ſuch have been uſually call'd chorographical and topographical maps: But when the map is any thing general, or is to contain any large tract of the earth or ſeas, ſuppoſe, for inſtance, one of the four quarters of the world,

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as they are call'd; in that caſe, when the projection or repreſentation is upon a plane, the parts muſt neceſſarily be diſtorted; as one way contracted beyond the truth, and another way dilated: ſo as to give no juſt idea of the whole. Nor can this diſtortion be poſſibly avoided, when any conſiderable part of a ſpherical ſurface, by any projection whatever, is to be repreſented upon a plane. It is true, this diſtortion is always regular and according to certain laws: So that knowing the nature of the projection, it may tolerably well be allow'd for: But to do this ſcientifically, and as it ought to be done, requires much skill and accuracy in the maker, as well as good proficiency and experience in the peruſer; and therefore not ſo proper for an introduction to learners in the rudiments of geography; young minds being apt to receive wrong notions and prejudices from them; at leaſt they cannot be rightly and eaſily inſtructed by them.

To obviate this inconvenience, geographers have contrived and conſtructed the terreſtrial globe, on which they endeavour to delineate all the parts of the earth's ſurface in their natural ſtate, as to longitude, latitude, diſtance, bearing, magnitude, &c. which being a true and genuine repreſentation of the whole ſuperficies of the earth, as far as it is hitherto known, is the beſt adapted for conveying juſt notions to young minds, and for preventing all falſe conceptions and prepoſſeſſions. After the firſt rudiments of geography have been imbibed from hence, they will then be prepared for the uſe of plain maps; and will afterwards find, that large projections of particular countries, kingdoms and provinces *in plano*, will be of excellent ſervice to them for their farther improvement in this uſeful and neceſſary ſcience. Nor will they now be in any danger of being miſled by ſuch maps, tho' they are not ſuch juſt and natural repreſentations of the terreſtrial globe.

Now the ſame conveniencies, that may be derived from the whole globe, may, in proportion, be had from any notable portions of it; as an hemiſphere, a quadrant, a ſextant, an octant, or other part; but with this advantage beſides, that theſe partial ſpherical maps will not only be much leſs cumberſome, and more managable than a whole globe, but may be made much more accurate and particular, as being capable of being formed to a much larger diameter than a globe can be conveniently made to. The maps may be firſt printed on a plane, as is uſual in the common globes, and

then

then paſted upon thin convex ſhells of paſtboard formed to the intended radius. The forming of theſe ſpherical coats of paſtboard will be matter of no great difficulty, even to as large a diameter as ſhall be deſired; but the chief art will be required in projecting the maps *in plano*, after the ſimpleſt and exacteſt manner; ſo as that they may adapt themſelves, with as little error as poſſible, to a ſpherical ſurface: For, a plane ſurface cannot be converted into a ſpherical ſurface without ſome error. The beſt method of doing this, with the leaſt poſſible error, Mr. *Colſon* thinks will be as follows:

Inſtead of the uſual ſlips or guſſets, as is the manner of globe-makers, comprehended between two meridians at ſome diſtance, and formed only tentatively and mechanically, without the help of any juſt theory, we may divide the whole ſpherical ſurface into parallel portions, or zones; that is, into parts terminated by two parallels to the equator, at the diſtance ſuppoſe of 10 degrees. As if the firſt of theſe portions, or zones, were at the equator itſelf, and extended to five degrees of latitude on each ſide of that circle, the 2d zone would be at the parallel of 10 degrees of latitude, and would extend to five degrees of latitude on one ſide, and to 15 dregrees of latitude on the other ſide of that parallel; and ſo of the ſucceeding zones.

Now we may conceive the firſt of theſe portions, or zones, to be converted from a ſpherical to a plane ſurface in the following manner, without ſenſible error. Let the middle line of this zone, that is, the equator, continue in its ſituation, and let the ſegments of the meridians on each ſide be conceived to unbend themſelves gradually, till they are extended into right lines perpendicular to the equator: Then will that, which was before a zone, or portion of a ſpherical ſurface, with a ſmall alteration, become a portion of a cylindrical ſurface, circumſcribed about the ſphere; whoſe breadth is every where equal to 10 degrees of the ſphere; and whoſe circumference is equal to the equator: And thus every parallel to the equator, as far as that of five degrees of latitude on each ſide will be ſtretched and extended into a circle, as large as the equator; but they will all keep the ſame diſtance from one another, and from the equator, that they had before. This extenſion, or alteration, will be every where regular and uniform, and will be but very little, even where it is moſt: For, the leaſt of theſe circles which is the parallel of five degrees of latitude, has the

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ſame proportion to the circle it is ſtretched to, or the equator, as the ſine of 85 degrees has to the radius, or as 9961947 to 10000000, which approaches very near to a ratio of equality: And now it will be eaſily conceiv'd, that without undergoing any other alteration, or diſtortion, this portion of a cylindrical ſurface may be rectified, or extended into a plane parallelogram, whoſe length will be equal to that of the equator, and whoſe breadth will be equal to an arch of 10 degrees of the ſame equator.

And conſequently, by an operation that will be juſt the reverſe of this; if upon a plane we delineate ſuch a parallelogram as this, we may then lay down all the places contained therein, very exactly, in their proper ſituation of longitude and latitude; and then apply its middle line or equator to that of a globe of a due magnitude, which will then become a portion of a cylindrical ſurface, circumſcribed about the globe. Then by preſſing it cloſe to the body of the globe; we ſhall cauſe it to contract itſelf a very little, but regularly; which contraction will be only according to longitude, and not at all according to latitude: And then the cylindrical ſurface will be changed into that of a ſphere, and become the firſt ſpherical zone before deſcribed, with all its delineations in their due poſition, without ſenſible error.

In like manner in the ſecond ſpherical portion or zone, comprehended between the parallels of 5 and 15 degrees, whoſe middle line is the parallel of 10 degrees, we may conceive the ſegments of the meridians to unbend gradually on each ſide, and extend themſelves into tangent right lines, which therefore will form a ſegment of a conical ſurface, ſtill touching the globe in the parallel of 10 degrees of latitude. The axis of this cone will coincide with the prolonged axis of the globe; and the ſide of the cone, which is to be eſtimated from the vertex to the circle of contact, will be the co-tangent of the latitude, or the tangent of 80 degrees. Now this portion of a conical ſurface may eaſily be conceived to be unroll'd, or expanded into a plane ſurface, without undergoing any other alteration; and then it will become a portion of a ſector of a circle; which portion will have for its length or middle line an arch of a circle, deſcribed with the aforeſaid tangent, as a radius; whoſe length will be the ſame as the parallel of contact, and its breadth equal to an arch of the equator of 10 degrees, as before. This ſegment of a ſector of a circle, ſo produced, may, there-

therefore, be eaſily deſcribed *in plano*, and within it may be inſerted all the places belonging to it, according to their longitude and latitude: Then it muſt be applied to the globe; ſo as that its middle line ſhall coincide with the parallel of 10 degrees. Then by preſſing, it may be bent to the ſurface of the globe, every meridian to its reſpective repreſentative, by which it will uniformly contract a little according to longitude, but not at all according to latitude. And thus the globe will be cover'd as far as 15 degrees of latitude.

The next zone, or that belonging to the parallel of 20 degrees, may be conſtructed *à priori* in the following manner: Upon a plain paper, with radius equal to the tangent of 70 degrees, deſcribe an arch, whoſe length is equal to that of the parallel of 20 degrees; as alſo two other concentric arches on each ſide, at a diſtance from the middle arch, equal to an arch of 5 degrees: This will be the requir'd ſegment of the circular ſector, in which are to be inſerted all the places belonging to it, according to their longitude and latitude. Then the middle line or arch is to be applied to the parallel of 20 degrees upon the globe, and the ſegment of the conical ſurface, thence ariſing, is to be duly contracted as before, or preſs'd cloſe to the globe; by which means this zone will alſo be compleated. And in the ſame manner we are to proceed to the ſucceeding zones, till the whole globe is cover'd: And the method will not differ in any material circumſtance, if inſtead of a whole globe, we are to conſtruct any part of it only, or what Mr. *Colſon* here calls a ſpherical map.

To reduce this theory to practice, and as a ſpecimen of ſpherical maps, Mr. *Colſon* has conſtructed a terreſtrial hemiſphere to a diameter of near 15 inches; to which he has given the name of the *Britiſh* hemiſphere; becauſe it has *Great-Britain* in the centre, or rather at its vertex. It is, therefore, adapted to the meridian and horizon of *London*, and exhibits one half of the earth's ſurface, as it lies round about this City; which is vaſtly the moſt conſiderable part of the whole earth's ſuperficies. The longitude and latitude of places are here eaſily known by inſpection, and their bearing and diſtances may be nearly eſtimated: And all the delineations are as accurate and particular as this ſmall radius would permit: He, therefore, conceives it may be no unfit inſtrument for inſtructing beginners, or for initiating young minds in the firſt rudiments of geography.

A

A Copy of an ancient Chirograph, *or conveyance of Part of a* Sepulchre, *cut in Marble, lately brought from* Rome, *together with some Observations thereon, by Mr.* Gale. Phil. Tranſ. N° 441. p. 211.

The inſcription at full length is as follows;

Diis Manibus
Marci Herennij
Proti, vixit annos viginti duos
Menſes duos, dies quinque, fecerunt Parentes
Marcus Herennius Agricola &
Herennia Lacena filio.
Chirographum. Ollaria numero quatuor
Cineraria quinquaginta tria intrantibus parte lævâ quæ ſunt in monumento
Titi Flavii Artemidori, quod eſt viâ
Salariâ in agro Voluſi Baſilides
Ientibus ab urbe parte ſiniſtra, Donationis cauſa Mancipio accepit
Marcus Herennius Agricola de Tito Flavio
Artemidoro Seſtertio nummo uno Libripende Marco
Herennio Juſto, Anteſtatus eſt Tiberium
Julium Erotem: Inque vacuam
Poſſeſſionem earum ollarum
Et cinerariorum Titus Flavius Artemidorus Herennio Agricolæ ire
Aut mittere, oſſaque inferre permiſit, ſacrumque quotiens facere vellit Herennius Agricola
Heredeſve ejus permiſit, Claviſve
Ejus monumenti poteſtatem facturum ſe dixit, dolumque malum
Huic rei abeſſe afuturumque
Se hæc recte dari, fieri præſtarique ſtipulatus eſt Marcus Herennius
Agricola, ſpepondit Titus Flavius
Artemidorus Actum XVIII. Kalendas Januarii
Caio Calpurnio Flacco, Lucio Trebio
Germano Cos.

This marble, lately arriv'd from *Rome*, and now repoſited in Sir *Hans Sloane's Muſæum*, is a very valuable piece of antiquity, as it exhibits a complete *formula* of a chirograph, or conveyance of one part of a burying place from one family to another, but neither of them of any note, ſeeming by their *agnomina* to have been only *Liberti*, or deſcended from ſuch. *Agricola*, it is true, is a *Roman* name, but thoſe of his wife *Lacena*, and his ſon *Protus*, are both *Greek*.

By this chirograph (line 7, 8, *&c.*) *Herennius Agricola* obtains from *Titus Flavius Artemidorus* a right to 4 *ollaria*; which were niches or repoſitories, wherein they placed *cineraria*, urns or veſſels of ſtone or earth, containing the aſhes of the dead; and were here 53 in number.

Fabretti, it is true, in *Inſcript. ant. in ædibus pat. p.* 16, 17, takes the *cineraria* to have been niches for receiving and keeping *ſtone-urns*; but *Gutherius de jure Manium lib.* 2. *cap.* 24, tells us, that *oſſuariæ ollæ à cinerariis in eo differunt, quod hæ cineres, illæ oſſa exciperent.* Beſides, if they were niches, or the ſame as *ollaria*, the mentioning of them as in this inſcription, would be an unintelligible tautology; and *Spon*, in his *Miſcellan. Antiq. Erudit. p.* 290, gives us the following inſcription, which ſeems to put the matter out of diſpute.

Romæ in operculo Vaſis.

CINERARIUM
GEMELL. III AELI
MARCI ET PHILIPPI.

From both which authorities it is evident, that the *cineraria* were veſſels and not repoſitories for them.

This monument was ſituated on the left ſide of the *via Salaria*, which ran to the north-weſt of *Rome* from the *Porta Collina*. It ſtood in the ground of *Voluſius Baſilides*; and the conſideration for the conveyance of it, is one ſeſterce. It is very uſual in ſepulchral inſcriptions to find the monument of one family in the field of another; the proprietor of the monument reſerving the right of that to himſelf when he ſold the ground; or purchaſing ſo much ground from the owner as was ſufficient for erecting the monument. All ſepulchres, when once a body was interr'd therein, were eſteem'd as religious and ſacred, and were not to follow the poſſeſſion of the field.

Mille

Mille pedes in fronte, trecentos cippus in agrum
Hic dabat, hæredes monumentum ne ſequeretur.
Hor. I. Sat. 8.

Line 11. *Baſilides* is a blunder for *Baſilidis* in the genitive caſe.

Line 11, 12. The words *donationis cauſa mancipio accepit M. Herennius Agricola de Tito Flavio Artemidoro* IIS SN. I. are to be read *ſeſtertio nummo uno*, as is evidently demonſtrated from the following inſcription, where you have alſo the reſt of the words of this form of conveyance. There is likewiſe in *Gruter* p. DCCCCLVI. 4. an inſcription wherein the words *ſeſtertio nummo uno* are expreſs'd at length.

HOC. MONVMENTUM. SIVE
SEPVLCHRUM. CUM AREA. SVA
T. FVFICIUS FELIX. DE
JVLIA RVFINA. DONATIONIS.

CAVSA MANCIPIO ACCEPIT
IIS. N. UNO. QUOD COMPARAVIT
FVFICIÆ AMPHATÆ
CONJVGI CARISSIMÆ, &c.

They were *verba ſolennia donationis vel alienationis cauſâ quæ fiebat per mancipium.* This mancipation was often a fictitious ſale of a thing to make the donation of it valid, as in this caſe. And the mention of one ſeſterce given for it, is only *dicis gratiâ*, much like our form in leaſes, *in conſideration of 5 ſhillings in hand paid.* Frequent examples occur of this practice, as in the inſcription juſt now quoted from *Fabretti, inſcript. ant. in æd. pat. p.* 50. and others in the ſame author; and in *Gruter* (p. DCCCCLVI. 4. and MLXXXI.) which latter is a compleat *formula* of a like ſepulchral conveyance as this, but of a later date, and not ſo well preſerv'd; it being executed when the Emperor *Trebonianus Gallus*, and his ſon *Voluſianus*, were Conſuls *A. D.* 252; and this in Sir *Hans Sloane*'s poſſeſſion, probably (as ſhall be ſhewn farther on) during the reign of *Septimius Severus.*

Line 15. LIBRJ PENDE is cut in the marble as 2 diſtinct words, as here repreſented; tho' in reality it ſhould be

but one; and ſignifies the perſon that weigh'd or counted over the money to the ſeller: It ſhould be read LIBRIPENDE, than whom there could not be a more proper witneſs to the purchaſe. At the beginning of the *Roman* ſtate their money was uncoined, and call'd *æs rude*, or *grave*, therefore, paid by weight; whence comes the word *libripens*. Under *Servius Tullius*, their ſixth king, it began to be coined, and paid by tale; but the perſon who counted it over to the receiver ſtill retain'd his primitive appellation. Almoſt every conſiderable town had its *libripendes*, perſons of ſkill in money affairs, to determine controverſies about the value of it.

An inſcription in *Gruter* (p. MCXV.I.) is a ſtrong proof of this: It was found at *Nola* in *Campania*, and ſhews they had 2 *libripendes* there appointed by publick authority.

T. VEDIVS. T. F.
T. VITORIUS. CN. F.
II. VIRI
LIBRIPENDES
EX. DD.

They had this name; *quia libram æneam tenebant, quâ nummos penderent.*

—— *Librâ mercatus & ære.* Hor. Epiſt. II. 2.

And hence we have the words *ſtipendium*, *diſpendium*, *expenſæ*, and the like. In *Apuleius's Metamorph.* Book X. is the following paſſage; — *ſed ne fortè aliquis, inquam, iſtorum quos offers aureorum, nequam vel adulter reperiatur, in hoc ipſo ſacculo conditos eos annulo tuo prænota, donec altero die* Nummulario *præſente comprobentur*; where this *Nummularius* ſeems to be the ſame as the *libripens*, who was generally call'd in to count over and examine the money at payments for purchaſes, tho' ſometimes a private perſon or friend to the parties might probably perform this office for them, and be an evidence, upon occaſion, to the facts: To which end alſo they us'd to bring another witneſs, as *Herennius Agricola* does here, who was one *Tiberius Julius Erotes*, and ſometimes they added 5 more. The form and manner of doing it was by aſking a by ſtander, *licetne anteſtari?* If he conſented, the demandant touch'd, or pull'd, the lower part of his ear, as a *memorandum* of what paſſed: Whence *Horace* in his IXth *Sat.*

—— *caſu*

—— *casu venit obvius illi*
Adversarius, & quo tu turpissime? magnâ
Exclamat voce, & licet antestari? *Ego vero*
Appono auriculam.

By the law of the XII tables, if he that was call'd to testify in this manner, or the *libripens* refused afterwards to give his evidence in the case, they were adjudged infamous. *A. Gell. l.* XV. *c.* 14.

Line 18, *Earum Ollarum* seems to be a mistake for *eorum ollariorum.*

Line 20 to 27, are covenants usual upon this occasion, as may be seen in the like sepulchral contracts, particularly, the before mentioned in *Gruter* (p. MLXXXI.I.) and many other donations and orders about monuments in his voluminous collection; as also in *Fabretti* and *Reinesius.*

Line 28, S E seems to have been a blunder of the *marmorarius* for S I B I, *se dari* being entirely ungrammatical. But in the contract aforesaid, given us by *Gruter*, the words run *de ea re dolum malum abesse, afuturumque a te, hærede tuo, & ab his omnibus ad quos ea res pertinebit, hæc sic rectè dari, fieri, præstarique stipulatus est*; which inclines Mr. *Gale* rather to believe, that *se* in the present case ought likewise to have been *sic.* There are several palpable mistakes in it, as in line 23, *vellit* for *velit*, and in line 24, *clávisve* for *clavisque.*

The *Roman* lawyers tell us, that *stipulatio* was *interrogatio certis, solennibusque verbis concepta; & apta consentaneaque responsio, veluti* spondes? spondeo. Dabis? Do. This is fully confirmed both in this and the Gruterian contract (p. MLXXXI.I.) in the former *stipulatus est Marcus Herennius Agricola: spepondit T. Flavius Artemidorus*: In the latter, *stipulatus est Licinius Timotheus: spepondit Statia Irene.* The learn'd Mr. *Mattaire* observes from *Aulus Gellius*, lib. VII. c. 9. that ancient authors used *e* instead of *o*, in those verbs which have a reduplication in the preterite tense, as *memordi, pepoſci, spepondi*, for *momordi, popoſci, spopondi*, us'd by more modern writers: So that *spepondit* is no mistake, but an archaismus, as may the word *ientibus* in the 12th line; tho' it has not been remarked, as the latter was. *Iens* in the nominative case was us'd more than once by *Cicero*; and tho' he declines it, as all other authors now in being, *euntis, eunti, &c.* yet it might originally have been declined *ientis, ienti*: But as there is now

NO

no authority extant to warrant it, this muſt paſs as mere conjecture.

Line 32, there are no ſuch names to be found in any of the *faſti Conſulares*, as *C. Calpurnius Flaccus*, and *Lucius Trebius Germanus*: So that they muſt have been, not the *Conſules ordinarii* of the year, but the *Suffecti*. It is very ſtrange that the *Romans* ſhould ſo long adhere to this troubleſome and uncertain method of computation by the years of their Conſuls; ſince they had frequently ſeveral pairs of them in the ſame year, eſpecially after the Government became imperial: Some reckon'd by the ordinary Conſuls, who came into their office upon the 1ſt of *January*, about 600 years after the building of *Rome*: For, till that time the month of their entring upon that dignity was not fixed; and others computed by the *Suffecti*, who might come in ſeveral months after, as vacancies happen'd, or as they were appointed by the emperor; tho' their names were ſeldom inſerted in the *faſti*. Beſides this, it was impoſſible for any man to remember how many years were elapſed from the preſent time upwards, to ſuch and ſuch Conſuls, without tables of their ſucceſſion, or having recourſe to ſome other *æra*, as the A. V. C. *anno urbis conditæ*, which they do not ſeem to have much regarded.

In *Gruter* (p. XLVI. 9.) there is a long inſcription, mentioning *Trebius Germanus* (tho' not as Conſul) in the reign of *Septimius Severus*; and another (p. CCCLXXXII. 7.) of *C. Calpurnius Flaccus*: If theſe men were the Conſuls here referr'd to, as they might be, the age of the marble in Sir *Hans Sloane*'s poſſeſſion will be aſcertained within a few years.

The ſtone is turned with an arch a top; the whole length of it is 27 inches and $\frac{1}{2}$; the breadth at the bottom 10 inches and $\frac{1}{2}$; and at the baſe of the arch 12 inches and $\frac{1}{4}$, it widening gradually upwards. The letters are cut in a ſmall indifferent character; that of the E and the F are remarkable, being always formed in this manner E *f*. It was probably placed over or between the 4 niches, or *ollaria*, granted to M. *Herennius Agricola* in this monument by *T. Flavius Artemidorus*, in order to declare and aſſert the right and poſſeſſion of them to the former and his family, till they were all filled.

Of

Of the Revolutions, which ſmall pendulous Bodies will, by electricity, make round larger ones from Weſt to Eaſt, as the Planets do round the Sun; by Mr. Stephen Gray. Phil. Tranſ. N° 441. p. 220.

MR. *GRAY* made ſeveral new experiments upon the projectile and pendulous motion of ſmall bodies by electricity, whereby ſmall bodies may be made to move about larger ones, either in circles or ellipſes, that are either concentrical or excentrical to the center of the larger bodies, about which they move; ſo as to make many revolutions about them; and this motion will be conſtantly the ſame way that the planets move about the ſun, *viz.* from the right to the left, or from weſt to eaſt: But theſe little planets, if they may be ſo call'd, move much faſter in the *apogean*, than in the *perigean* parts of their orbits; which is directly contrary to the motion of the planets about the ſun.

The Conſtruction of a Quickſilver Thermometer, as alſo Obſervations on the Eclipſes of Jupiter's *Satellites, made at* Petersburgh, An. 1731, 1732; *by* M. Joſ. Nic. De l'Iſle. Phil. Tranſ. N° 441. p. 221.

M. *De l'Iſle*, in order to have ſurer grounds for his experiments in natural philoſophy in *Ruſſia*, and to compare them with thoſe of other countries, applied himſelf in winter 1733 to the conſtruction of mercurial thermometers, regulated by the expanſion of that fluid proportionably to its bulk: This expanſion is, it is true, not very perceptible, conſidering that Dr. *Halley* in his experiments made upon it upwards of 40 years before (as related in *Phil. Tranſ.* N° 197.) found that the ſaid expanſion, by the heat of boiling water, was no more than $\frac{1}{74}$ part of the bulk of mercury, the experiment having been tried in the months of *February* and *March*, when the weather was cold enough, tho' it did not freeze.

M. *Amontons* likewiſe relates in the *Memoirs* of the *Royal Academy* for 1704, that this expanſion of the mercury is but $\frac{1}{11\frac{1}{5}}$ part of its bulk from the greateſt heat to the greateſt cold felt at *Paris*. But M. *De l'Iſle* found in the great cold they had at *Petersburgh* on the $\frac{16}{27}$ of *January* 1732-3. in the morning, that the bulk of the mercury was condenſed almoſt $\frac{1}{150}$ part of the extent it had in boiling water. The cold they had

had that day, the wind being at eaſt, was one of the ſevereſt that ever was felt there.

M. De l'Iſle had his new thermometers made of a good large ſize, and in ſuch manner, that having divided in each the whole quantity of mercury it contains, into one hundred thouſand parts; and having marked the extent of the bulk of that mercury in boiling, he can at any time ſee on the diviſions of theſe thermometers, by how many parts the bulk of the mercury is condenſed thro' the preſent temperature of the air. And tho' he had made four of theſe thermometers, which differ very much as to ſize, and the quantity of mercury they contain, yet they agree within a very few of theſe parts. As pure mercury is of the ſame nature every where, and not liable to any alteration from being incloſed in a tube; and as it is probable, that taking it equally purified, it will in different countries be ſubject to the ſame expanſion, if expoſed to the ſame degree of heat: For this reaſon he is purſuaded, theſe thermometers may very well ſerve to compare the temperature of different countries; the rather, as he found by experience, that theſe thermometers may be rendered fit enough to mark ſenſibly the increaſe or diminution of the bulk of the mercury, within one or two parts out of the 100,000 contained in the whole bulk. This ſort of thermometers has alſo this advantage, that as they mark the proper expanſion of the mercury in each temperature of air, they may ſerve to ſhew every moment the correction, to be made in the height of the mercury in ſimple barometers; which will ſerve for reducing them to the height they would have in an equal temperature of air: And one might, for this end, chuſe and agree upon the heat of boiling water, as a fixed term, which in all appearance will be the ſame all over the world. If the *Royal Society* ſhould approve of this new conſtruction of thermometers, and order ſome of their members to make the like, we might hereafter be able exactly to compare the temperature of *England* with that of *Ruſſia*, and other places where the like thermometers ſhould be made.

M. De l'Iſle having in 1724 receiv'd of Dr. *Halley* a copy of his aſtronomical tables among which are thoſe of the four ſatellites of *Jupiter* by Mr. *Bradley*, he judged there could not be any better, till ſome method ſhall be found and explained geometrically to deduce from the laws of gravity, the effects of the mutual attraction of theſe ſatellites on one another

another, and with relation to *Jupiter*: But as he could not hope this would be done so soon, he took the pains again to calculate new tables upon those of Mr. *Bradley*, by reducing the tables of the four satellites into the same form with those Mr. *Pound* has made of the first satellite only. These tables being thus made easy, he has hitherto used them for comparing observations; and his brother has taken the pains, since the drawing up those tables, in the said manner, to calculate a year beforehand, all the eclipses of the four satellites. M. *De l'Isle* commonly sent those calculations to his correspondents to prepare them for observations, and some years of those ephemerides have been published in the little Gazette of *Literature of Leipsick*, published in *High Dutch*. His brother prolonged those calculations to the month of *January* 1737.

Here follow the observations on *Jupiter*'s satellites, which were made at *Petersburgh*.

N. St.	true time.	
1731 *Dec.* 6.	17^h 3′ 5″	The immersion of the first satellite difficultly observ'd with a reflecting telescope of five foot: The true time was found only by means of two clocks.
1732 *Jan.* 4.	13 30 56	Immersion of the 2d satellite by the reflecter, doubtful to a few seconds: *Jupiter* not being well defined, nor sufficiently high. The true time adjusted by two clocks.
9.	18 33 7	Immersion of the 4th satellite by the reflecter: The sky not very serene; and the true time adjusted only by two clocks.
1732 *Jan.* 9.	20 25 0	The other satellites disappearing by the day-light, the fourth was not yet emerged out of the shadow; the telescope the same.
Feb. 22.	13 25 34	The first satellite, just entering the shadow, was still visible, when a mist cover'd *Jupiter*.
	13 26 34	*Jupiter* being uncover'd, the first satellite did not now appear

	N. St. true time.			
				through the reflecting teleſcope. The true time was adjuſted only by two clocks.
1732	*Mar.* 8.	8h	22	20″ Immerſion of the 3d ſatellite by the reflecting teleſcope: The wind was ſomewhat troubleſome; the true time was adjuſted by two clocks.
	April 3.	8	46	23 Emerſion of the 1ſt ſatellite by the reflecting teleſcope; doubtful to a few ſeconds, by reaſon of the ſatellite's nearneſs to the planet.
	13.	7	20	30 Immediately after ſun-ſet, *Jupiter* becoming viſible to the eye, the 3d ſatellite appeared to be out of the ſhadow, and entirely clear by the reflecting teleſcope.
	20.	11	6	52 Emerſion of the 3d ſatellite by the reflecting teleſcope. The ſky ſerene.
	27.	15	13	0 The 3d ſatellite had probably been come out of the ſhadow for ſeveral minutes: For, the other ſatellites did not appear better than this, which was ſeen with the reflecting teleſcope thro' the miſt, *Jupiter* being low, and the *crepuſculum* ſtrong.
	May 10.	12	55	54 Emerſion of the firſt ſatellite by the reflecting teleſcope; ſky ſerene; the obſervation certain.
	26.	11	14	5 Emerſion of the firſt ſatellite by a teleſcope of 13 foot, cloudy,
	Dec. 24.	18	4	30 Immerſion of the firſt ſatellite by a 13 foot teleſcope; a good obſervation.

Expe

Experiments on perforating the Thorax, *and the Effects thereof in reſpiration; by Dr.* William Houſton. Phil. Tranſ. N° 441. p. 230. *Tranſlated from the* Latin.

Exp. 1. APRIL 1728. Dr. *Houſton* with a narrow ſcalpel made a puncture on each ſide of the *thorax* of a ſmall dog, and yet the animal barked and howled no leſs ſtrongly than he had done before; and gave no ſigns of a faulty reſpiration: Whence ſome of the aſſiſtants ſuſpected, that the inſtrument had not been plunged deep enough into the cavity of the *thorax*: To remove this ſuſpicion, he made two other punctures; but the ſucceſs was entirely the ſame: For, both his voice and life continued ſound and entire from 9 o'clock in the morning, till about ſix in the evening, when he was kill'd, in order to obſerve what had happen'd.

Upon opening the *thorax*, the four wounds of the membrane, that ſurrounds the ribs eaſily appeared; ſo that every one of them had penetrated into the cavity of the *thorax*. But upon inflating the lungs, no default appeared in them, tho' the knife had penetrated almoſt an inch into their cavities; nor did the air expire faſter than it uſually does out of the ſoundeſt lungs.

Exp. 2. In *Auguſt* the ſame year, the Doctor made the ſame experiment on two puppies about four days old, by punctures on each ſide of the *thorax* in the one, and large wounds in the other, ſo that the lungs came to view on each ſide, yet they did in no manner ſubſide, but rather ſeem'd to be protruded externally. Both animals continued howling for $\frac{1}{4}$ of an hour, when an end was put both to their life and miſery.

Exp. 3. About the beginning of *November* the ſame year, the Doctor opened the *thorax* of a middling dog, by a large wound on each ſide: And paſſing his finger through the apertures into each cavity of the *thorax*, he felt the lungs collapſed in ſuch manner, that the ſpace between them and the membrane that ſurrounds the ribs was about an inch: Two other ſtudents of phyſick likewiſe paſſed their fingers thro' the apertures, and affirmed they perceiv'd the ſame thing: But upon looſing the dog, he immediately got up pretty chearful, ran through the room, and howl'd: No tents were put into the apertures, nor any thing upon the wounds before next day, when bolſters with turpentine were ap-

plied. The dog being kept for three days after was so far from losing his voice, that his howling became pretty troublesome, and at length being let loose, he ran away.

Exp. 4. *January* 14. 1729, the Doctor having procured a young dog, had a mind to open the *thorax*, and give a passage to the air, without wounding much the skin and muscles, to prevent a troublesome hemorrhage, and unnecessary pain. By means of the instrument called *trois quarts*, he introduced a goose-quill pipe into each cavity of the *thorax*, with a piece of leather fixed to it, done over with pitch, that it might stick to the skin, after first shaving it, and thus retain the pipe: But the moveable *cutis* soon extracted it out of the cavity of the *thorax*: And tho' he attempted the same thing, by new punctures, it always proved unsuccessful: Then he separated on each side by a long incision the cutis, and afterwards cut asunder the intercostal muscles, and penetrated into both cavities of the *thorax*, as appeared by the air violently rushing out:

The day following he put into the orifices tents of cork which being thicker at each end, and slenderer in the middle, required neither bandage nor plaister to keep them on: And yet the dog neither died, nor lost his voice, but eat, drank, and seem'd to be pretty well, only he could scarce lie on his side, on account of the uneasiness of the wounds, especially the tents. Upon pulling out these latter, a passage at times was given the air, nay, one side was blown into thro' the pipe. After the dog had continued so for two days without having his voice sensibly impaired, on the 3d day, both the tents were of a sudden forcibly expelled; and the air passing and repassing freely through the orifices made such a hissing, that the dog, partly frighten'd, as it seem'd, with the noise, and partly afraid of being further hurt, run away and hid himself under the bed. Upon taking him out from thence, the Doctor again put the tents into the wounds, but in a little time after they flew out again: And in this condition he liv'd from 10 o'clock in the morning till about five in the evening, when next day (being the fourth from the perforation of the *thorax*) he was strangled; he gave no signs of a faulty respiration before the end of the 3d day, at which time he was a little short-winded.

Upon opening the *thorax*, it contained a pretty large quantity of sanies on each side. The lungs were contracted into a narrow compass, and so far as the Doctor and such as were

were present could observe, they were whole on one side, and wounded on the other.

For farther satisfaction about this matter, Dr. *Van Swieten* at *Leyden* made the following experiment, at which Dr. *Houston* was present.

Exp. 5. *January* 25, 1729 having tied upon a board a middle-sized dog, and opened the *thorax* by making a large wound on each side; the dog did not lose his voice, and the lungs were so far from being collapsed that a lobule of them on each side burst out through the aperture; these lobules thus sticking out ceased not to dilate and contract; and what was most surprizing, their dilatation was synchronal to the contraction of the *thorax*, and *e contra*; upon blowing into the cavity of the *thorax*, the respiration was no ways hurt. After the dog had liv'd in this manner for half an hour, without having the voice or respiration sensibly impaired, the *thorax* was farther opened on one side, by cutting asunder a rib; and then it appeared, that the lungs which is greatly surprizing, contract, while the *thorax* dilates, and *e contra*. The dog likewise survived this operation, till after all the company were satisfied about the matter, he was condemned to be strangled.

The unexpected phenomena of these experiments made Dr. *Houston* try the following one in company with some friends; in order to examine whether the lungs always applied to the membrane of the *pleura*, whilst the *thorax* was entire.

*Exp.*6 After extending and tying down firmly upon a board a small dog, and raising up the skin, we cut a piece off with a pair of Scissars in that part of the *thorax*, where the ribs are least cover'd by the incumbent muscles. Upon wiping the blood, and stopping its flux with spirits of wine, we cut off with a pair of pincers all that cover'd the ribs and intercostal muscles; and at length we carefully separated the intercostal muscles themselves. The *pleura* being laid bare in this manner exhibited the following phenomena. During the dilatation of the breast, a kind of white body appeared to apply to the inside of the *pleura*, but upon its contraction, and the animal's expiration, that white body flying upwards gave place to a certain ascending red body, and then the breast immediately dilating itself, the red body descended again, and the white one succeeded in its room, and thus alternately. Upon this, we likewise bared of its muscles the upper inter-

stice

ſtice of the ribs, but in that place there appeared nothing but the white body. During the dilation of the breaſt, the *pleura* became concave in each part where it was bared, and during its contraction, a little, but ſcarce ſenſibly, convex.

After having ſufficiently viewed all this, and cut aſunder two ribs, we open'd the breaſt by making ſo large a wound that almoſt all the contents of that ſide appear'd to the eye: Upon this the animal no longer emitted a cry, tho' the *thorax* on the other ſide was untouch'd: The lobe was ſuddenly much collapſed; but did not entirely loſe its alternate motion of dilatation and contraction; and according to every one preſent its dilatation was ſynchronal to the contraction of the *thorax*, and *è contra*: At length the ventricle of the heart was penetrated for the finger to feel its muſcular force: And thus at once a period was put to the dog's reſpiration and life.

Dr. *Houſton* was formerly of opinion that man, or any other of the more perfect animals, (for, the caſe is otherwiſe with frogs, *&c.*) could not live upon admitting air into both cavities of the *thorax*: But the falſity of this opinion is ſufficiently evinced by the above-mentioned experiments; and at the ſame time they ſeem to be repugnant to the opinion of reſpiration generally receiv'd, namely that the lungs are dilated by the weight of the air entering thro' the larynx, when its preſſure is taken off their external ſuperficies by the dilatation of the *thorax*: But he thinks that this apparent repugnancy may be removed, or at leaſt leſſen'd in the following manner.

Let us ſuppoſe, that both wounds are double the aperture of the *glottis*; but that the lungs are entire, and deſtitute of all contractile force; and now let the *thorax* be dilated and filled by the lungs diſtended with air, then;

1. Upon the firſt contraction of the *thorax*, the air will eſcape through the *glottis*.

2. When the *thorax* is again dilated, the air will enter, in the proportion of its increaſed capacity; but it will not all enter thro' the *glottis*, nor all thro' the wounds, but through each aperture proportionally; and the quantity, entering through the *glottis*, will be to that entering through the wounds, as the aperture of the *glottis* to the ſum of the apertures of the wounds; or in the preſent caſe as 1 to 4. Therefore, the air, with which the lungs are now inflated, will be

be $\frac{1}{5}$ of what they would be inflated with, had the *thorax* remained intire.

3. While the *thorax* is contracted a 2d time, the air ſhould paſs out the ſame way it enter'd at in the preceding dilatation: And if the apertures continue without any alteration, the air, that enter'd thro' the wounds, ſhould paſs through them preciſely in the ſame time that the air, which enter'd thro' the *glottis*, paſſes through it: So that no air will remain between the lungs and the *pleura*, but every thing will be in the ſame ſtate, as at the end of the firſt contraction; and if $\frac{1}{5}$ of the uſual inflation of the lungs be ſufficient for the life and voice of the animal; or if it can dilate the *thorax* five times more than uſual, then nothing hinders but that upon opening the *thorax* in this manner the animal may live and utter his voice.

The Dr. ſuppoſed one thing entirely falſe, namely, that the lungs were without any contractile force; which would overturn the whole, were there not a compenſation ſome other way: But it is evident, that the aperture of the glottis may be contracted or dilated at the pleaſure of the animal, nay, is actually dilated, during inſpiration, but contracted during expiration, at leaſt when the voice is a forming; whence it happens that the ingreſs of the air can be ſo much aſſiſted by the dilatation of the *glottis*, as it is hindred by the contractile force of the lungs; and its egreſs ſo much hindred by the contraction of the *glottis* as it is aſſiſted by the contraction of the lungs: And an equilibrium may be preſerv'd, or the one or the other cauſe prevail, according as the animal may have this power of changing the *glottis*, or according as he uſes it: Which likewiſe agrees very well with the phenomena, for, when the dog howl'd, the lungs burſt out through the wounds; and while he was quiet, they enter'd in again, and never appeared.

As to the phenomenon of the lungs being obſerv'd to dilate while the *thorax* was contracted, the Dr. takes this to have been owing to no other cauſe than to the contraction of the muſcles of the abdomen with a vaſt and convulſive force, whereby they drove every thing upwards, and conſiderably leſſen the capacity of the *thorax*; whence the air, driven from the lower, tended to the upper parts of the lungs; and tho' the lobe (in reſpect of the whole) was contracted, yet in that part where it regarded the wound, it was dilated.

In

In *Exp.* 6. It is self evident that the white body was the lungs, and the red, the diaphragm; as also that the lungs fill the whole capacity of the *thorax*; and as is commonly suppos'd, apply their superficies to its membrane.

Yet there still remain other difficulties in this matter, to be remov'd by more accurate experiments, and cleared up by ingenious men.

Observations, astronomical, physical and meteorological, for the Year 1733, *at* Wittemberg; *by M.* Weidler. Phil. Trans. N° 441. p. 238. *Translated from the* Latin.

A Conjunction of *Saturn* and *Mars*: On *Feb.* 5, at 7h 30 min. in the evening, both these planets were seen in a streight line with *Bayer*'s E of *Pisces*, as represented in Fig. 23. Plate VI.

Saturn and *Mars* distant	2°	15′	0″
Mars and E of *Pisces*	1	12	30

Feb. 19. at 7h. 15 min. in the evening, the distance of *Mars* from *o* of *Pisces* towards the north was observ'd 1° 17′ 30.

The mildness of the preceeding winter in *February* and *March* occasioned epidemical, catarrhous fevers.

Mar. 14, the cold, felt that day sharper than ordinary, was brought by a very stormy north-east wind that blew 2 days before.

An observation of a solar eclipse made apart on the 2d of *May* O. S. is to be found in *Phil. Trans.* N° 433.

May 3, an *aurora borealis* is said to have appear'd.

May 5, the cold in the night injured the vines and walnuts, and likewise nipt the corn every where, in the lowest grounds, so that afterwards the ears had no corn in them.

June 26, in the evening, an *aurora borealis* appear'd, and it shot forth the usual pyramids about 12 o'clock at night.

Oct. 27. there again appear'd an *aurora borealis*; about 7 o'clock in the evening a black arch, covered with a white *fascia*, was observed under the north; at 8h. 30′ a white zone lying upon the black arch expanded insensibly higher, to about 40 degrees, and this lucid space continued quiet till 10 o'clock. After 10, pyramidal radiations shot forth swiftly every where from the white tract, and undulating clouds also roll'd along the heavens, but the entire white edge was sometimes observ'd to rise like a lucid wave towards the pole. This undulation of the

he ſhining matter, by pyramidal rays, much brighter than that hining tract, afterwards blending therewith, laſted till 11 o'clock: The whole ſhining matter gradually moved towards the eaſt, in he room of which ſucceeded black diſcontinued clouds towards he weſt. A little before 11 a white cloud ſhone for a longer ime near the north-weſt, and afterwards it became red, and its natter likewiſe had an undulatory motion. The ſcene vaniſh'd after 11 o'clock; the air, all the time of the appearance was calm; and the ſtars ſhone every where thro' the white tract: What ſeemed remarkable to M. *Weidler* was, that the very next day prov'd dark and rainy; as alſo that in the night after the 29th there blew an exceeding ſtormy wind; ſince at other times after the more remarkable *auroræ boreales* there uſually ſucceeds more ſerene weather.

The ſame phenomenon of an *aurora borealis* was likewiſe obſerv'd at *Stockholm* in *Swedland*.

An Account of the crooked and angular appearance of the ſtreaks or darts of Lightning *in* Thunder-ſtorms; *by Mr.* Logan. Phil. Tranſ. N° 441. p. 240.

MR. *Stephen Hales* in his *Statical Eſſays*, Vol. II. p. 291. mentions this phenomenon of the ſtreaks or darts of lightning in thunder ſtorms appearing crooked and angular, as a thing not hitherto accounted for: and therefore, he gueſſes at a ſolution of it.

The clouds are generally diſtinct collections of vapours, like fleeces; and therefore, the rays of light through them muſt paſs through very different denſities; and accordingly ſuffer very great refractions: From thence therefore, that appearance muſt undoubtedly ariſe. For, it is highly abſurd to imagine, that fire darted with ſuch rapidity can form any aſſignable cauſe deviate from a right line, in the manner it appears to us: And this, if duly conſidered may probably be found a plenary ſolution.

Obſervations on the aurora borealis *made in* England; *by* M. Celſius. Phil. Tranſ. N° 441. p. 241.

SEPT. 13, 1735, in the town of *Woodford*, 6 miles to the north-eaſt of *London*, at half an hour after 11 at night there appear'd a bright *faſcia*, almoſt parallel to the horizon; and its middle was judged to be under *n* of the *great bear*. At times another light ſhot along the great bear; but almoſt conſtantly cover'd γ and ζ.

Oct. 4. In *King-ſtreet, Bloomsbury, London.*

	h	′	″	
At	9	22	0	*p. m.* a ray or ſtream of light appear'd under the polar ſtar, perpendicular to the horizon.
	9	24	13	That ray diſappear'd.
	9	27	6	Two perpendicular rays ſhot forth 5 or 6 degrees from the north towards the eaſt.
	9	28	30	A whitiſh ray ſhot forth again exactly under the polar ſtar.
	9	30	0	That ray moved weſtward.
	9	31	0	It was ſeen under η of the great bear.
		31	27	It entirely diſappear'd.
	9	36	8	A ray aſcending perpendicularly by the polar ſtar, and α and β of the great bear.
	9			
	9	40	0	The ray ſeemed to move gradually under ξ of the great bear.
	9	44	0	No more rays appear'd: But whether there were any remains of light near the horizon, or in the weſt, M. *Celſius* could not ſee, by reaſon of the neighbouring houſes.

Oct. 11. in *London.*

	h	′	″	
At	10	37		*p. m.* There were two bright rays under and ε of the great bear.
	10	39	0	A ray between ε of the great bear and the polar ſtar.
	10	39	30	A ray in form of a pyramid above η of the great bear.
				Theſe rays had not any motion parallel to the horizon; but they entirely diſappeared.

Jan. 11. 1735-6 in *London*

	h.	′	″	
At	10	0	0	*p. m.* an indifferently bright arch, pale towards the edges, appear'd 16 degrees high; one of its ends deſcended eaſtward under η of the great bear: And lucid ſtreaks appear'd now and then over this arch.

Under

	h.	′	″	
At	11	13	0	Under this arch was another very bright tract parallel to it, 5 degrees above the horizon; in which tract there were rays that ſhot from the weſt towards the eaſt.
	11	15	0	This arch was very faint.
	11	17	0	The firſt arch became brighter, and the lower arch was almoſt blended with the upper, and broken in the middle.
	11	20	0	The whole arch was beſet with faint rays.
	11	21	0	One of the rays under the polar ſtar.
	11	27	0	No arch, nor rays, but here and there bright tracts.
	11	36	0	The light reach'd up to the polar ſtar, and ſomewhat higher.
	11	44	0	A lucid ray under the polar ſtar.
	12	11	0	The ſky was overcaſt with clouds, except one lucid ſtreak which appear'd 3 or 4 degrees to the eaſt of the north.

Feb. 16. in *Clare-hall Cambridge*

At a quarter paſt eight in the evening, the moon ſhining very bright, there appear'd 2 perpendicular ſtreams between the great and little bear.

April 3. at *London.*

	h.	′	″	
At	8	46	0	M. *Celſius* obſerv'd a lucid arch one degree broad, which extended along the northern crown, the *belt* of *Bootes*, the *coma Berences*, the leſſer *Lion* and *Cancer*, as far as the ſmaller dog.
	8	49	36	This arch quite diſappear'd: But at the ſame time he obſerv'd another broader and brighter arch under *Caſſiopeia*, 7 or 8 degrees high.

In the obſervations of *October* 4. and in the laſt, M. *Celſius* is certain as to the time of the clock: So that if others have obſerved the ſame phenomena, the longitude of places may be determin'd by them with greater exactneſs than by *Jupiter*'s ſatellites; which he takes to be the principal uſe that may be made of theſe obſervations, eſpecially in making maps of the northern countries, where theſe *lights* do more frequently occur.

Some Experiments made on mad Dogs, *with* Mercury; *by Dr.* Robert James. Phil. Tranſ. N° 441. p. 244.

THE following is an account of ſome experiments made on mad dogs with mercury, which Dr. *James* has ſome reaſon to believe is the moſt effectual preſervative, and perhaps, even a cure for the *hydrophobia*.

About *Michaelmas*, 1731, Mr. *Floyer* of *Hints* complain'd that he was afraid of a madneſs amongſt his fox-hounds: For, that morning one had run mad in the kennel. The Dr. took this opportunity of telling him, that he had long believ'd that mercury would, if tried, prove the beſt remedy againſt this infection: Mr. *Floyer* neglected this advice till the *February* following. Mean time he tried the medicine in *Bates*, commonly known by the name of the pewter-medicine; as alſo every thing elſe that was recommended to him by other ſportſmen, but to no purpoſe: For, ſome of his hounds run mad almoſt every day after hunting. Upon this he took his hounds to the ſea, and had every one of them dipt in the ſalt-water; and at his return, he brought his pack to another Gentleman's kennel, 6 miles diſtant from his own. But notwithſtanding this precaution, he loſt 6 or 7 couple of his dogs in a fortnight's time. At length in *February* Mr. *Floyer* tried the experiment the Dr. had recommended, upon 2 hounds that were mad, and both very far gone. They refuſed food of all ſorts, particularly fluids, ſlaver'd much, and had all the ſymptoms of a *hydrophobia* to a great degree. That night he gave 12 grains of turpeth mineral to each of the 2 dogs, which vomited and purged them gently. Twenty four hours after this, he gave each 24 grains; and after the ſame interval he gave 48 more to each. The dogs ſalivated very much, and ſoon after lapp'd warm milk. At the end of 24 hours more he repeated to one dog 24 grains more, and omitted it to the other. The dog, that took this laſt doſe, lay upon the ground, ſalivated extremely, was in great agonies, and had all the ſymptoms of a ſalivation rais'd too quick; but got thro' it: The other relapſed and died.

To all the reſt of the pack he gave 7 grains of the turpeth for the firſt doſe, 12 for the 2d doſe, at 24 hours diſtance, which was repeated every other day for ſome little time. The method was repeated at the 2 or 3 ſucceeding fulls and changes of the moon. From this time he loſt not another hound; and tho' ſeveral afterwards were bit by ſtrange dogs, the turpeth always prevented any ill conſequences.

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The Dr. and his friends tried the ſame thing upon a great many dogs, and it never fail'd in any one inſtance: Tho' dogs, bit at the ſame time and by the ſame dogs, have run mad after moſt other medicines had been tried.

As to the experiments on mankind, the Dr. had opportunities of making but 3.

The firſt was on a girl about 14 years of age: The calf of her leg was torn by a mad dog in ſuch a manner, that the ſurgeon was obliged to uſe means to prevent a mortification from the bite: She was vomited by the turpeth: Three days before the next change of the moon, the vomit was repeated, and again the very day of its changing. The ſame method was purſued the next full moon. The girl was very well.

The ſecond was on a boy of about 10 years of age. He had 4 holes in one of his legs, made by a mad dog. The turpeth was given as above, and the wounds dreſs'd with digeſtives, and he continues well.

The third caſe was that of a young man, of about 18 years of age: The bite was upon the hand. A great number of dogs were bit at the ſame time in the town where he liv'd. About 6 days after the miſchief was done, ſeveral dogs, that had been wounded, ran mad; upon which he applied himſelf to Mr. *Wilſon*, apothecary in *Tamworth*, to whom the Dr. had communicated the ſucceſs of the turpeth in this caſe. The young man was at this time very melancholy and dejected, had tremors, and ſlept very little for ſome nights before, tho' he was not apprehenſive that the dog which bit him was mad. He had a dry ſcab upon his hand: Upon applying to Mr. *Wilſon* he was vomited with *Vin. Benedict.* ℥ ij.

The next thing he took was made according to the following preſcription.

℞ *Turpeth Min. gr. XII. Lap. Contrayerv.* ℥ i. *Ther. Androm. q. ſ. M. F. Bol.* N° 3, *ſumat unum ſingulis noctibus horâ decubitûs ſuprabibendo Julap. ſeq. cochl. IV.* ℞. *aq. Rut.* ℥ vj. *Theriac.* ℥ ij. *Syr. Pæon. c.* ℥ iſs. *Tinct. Caſtor.* ℥ ij. *M. F. Julap.*

Upon taking theſe the patient ſweat very much, and had 2 looſe ſtools every day after them. His tremors went off, and he ſlept better. After this he went into the cold bath, and continued perfectly well.

But what is remarkable in this caſe is, that the wound ran a thick digeſted matter after this method, and threw off the ſcab like an *eſcar*; after which it heal'd of itſelf.

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The Dr. makes an obſervation or two on the antiquity of this diſeaſe, which he the rather chooſes to do; becauſe *Cælius Aurelianus*, in his account of it, does not ſeem to build ſo much upon the authority of *Homer*, as, in the Dr's opinion he might have done. He quotes, it is true, a paſſage out of the 8 *Iliad*, where *Teucer* calls *Hector* κυνα λυσσητηρα, but does not ſeem to think this ſufficient to prove that *Homer* was acquainted with this madneſs. But he omits 2 more paſſages in the ſame author, which, joined with this, amount to a demonſtration that *Homer* was by no means ignorant of it. The firſt is in the 9th *Iliad*, *l.* 237 where *Ulyſſes* is upon his embaſſy to *Achilles*, and deſcribes to that hero the diſtreſs of the *Grecian* army thro' his abſence; and when he has painted *Hector* as terrible as he can, he compares his fury to the rage of a mad dog.

——— Ἕκτωρ δε μεγα δεινεϊ βλεμεαινων
Μαινεται εκπαγλως, πισυνος Διϊ, ουδε τι τιει
Ἀνερας ουδε Θεους, κρατερη δε ε λυσσα δεδυκεν. *i. e.*

——— *Hector vero valde trucibus oculis adſpiciens*
Furit terribiliter, fretus Jove: Nec quicquam honorat
Viros neque Deos; ingens autem ipſum rabies invaſit.

If *Homer* had deſigned to deſcribe a mad dog as a phyſician, he could not have expreſſed his looks by a more proper term than Βλεμεαινων. It muſt alſo be conſider'd, that this diſcourſe is directed to *Achilles*, who, having ſtudied phyſic under *Chiron*, was conſequently more capable of receiving an idea of the miſchief *Hector* did to his countrymen by this metaphor.

In the 13th *Iliad*, *Hector* is again call'd Λυσσωδης by *Neptune*.

It muſt be obſerved that λυσσα, λυσσητηρ and λυσσωδης, can properly and in their natural ſignification be applied to no other madneſs than that peculiar to a dog, tho' metaphorically it may, as in the inſtances the Dr. has given, as alſo in *Sophocles* and *Euripides*. The term λυσσα or λυττα is uſed by *Ariſtotle*, *Galen* and *Dioſcorides*, to ſignify the madneſs of dogs: And Λυσσοδηκτος is uſed by the laſt mentioned author to ſignify a man bit by a mad dog. Λυσσαω is uſed by *Aretæus* in this ſenſe, and Λυττωσαις by *Plutarch* to expreſs the ſame thing.

What the Dr. would infer from this is, that *Homer* was certainly acquainted with the madneſs of dogs; and if dogs in his days ran mad, it is probable they would bite men; and if ſo,

to

to be ſure, an *hydrophobia* would be the conſequence: And yet *Plutarch* will have it, that it was firſt taken notice of in the days of *Aſclepiades*, famous for his practice in *Rome* before the death of *Mithridates*.

Another ſtrong evidence of its antiquity is that inſtinct which directs every dog to avoid a mad dog upon ſmelling, ſeeing, or even hearing him. If this be not inſtinct, it is reaſon, and that in a higher degree than we ourſelves can pretend to. Now inſtinct muſt be coeval with the creation, or at leaſt the fall; and therefore, madneſs muſt not be much younger.

A Catoptric Microſcope; *by Dr.* Robert Barker. Phil. Tranſ. N° 442. p. 259.

THO' microſcopes, compoſed of refracting glaſſes only, have been vaſtly improved, as to their effects of magnifying; yet they have been attended with ſuch great inconveniencies, that their application to ſeveral arts, in which they might be very convenient, is not ſo common as might be expected; and mankind have reap'd but a ſmall part of the advantage obtainable from ſo ſurpriſing and uſeful an inſtrument.

Among the inconveniencies mentioned, the following are the moſt conſiderable.

1. That in order to magnify greatly, it is neceſſary the object glaſs be a portion of a very minute ſphere, whoſe *focus* being very ſhort, the object muſt be brought very near; it will, therefore, be ſhaded by the microſcope, and not be viſible by any other light than what paſſes thro' itſelf: In this caſe, therefore, opaque objects will not be ſeen at all.

2. Objects, illuminated this way, may be rather ſaid to eclipſe the light, than to be truly ſeen, little more being exactly repreſented to the eye, than the out-line; the depreſſions and elevations within the out-line appearing like ſo many lights and ſhades, according to their different degree of thickneſs or tranſparency; tho' the contrary happens in ordinary viſion, in which the lights and ſhades are produced by the different expoſure of the ſurface of the body to the incident light.

3. Small parts of large objects cannot eaſily be applied to the microſcope, without being divided from their wholes, which in the caſe of vivi-ſection defeats the experiment, the part dying, and no more motion being obſerv'd therein.

4. The *focus* in the dioptric microſcope being ſo very ſhort, is exceeding nice, the leaſt deviation from it rendering viſion turbid; therefore, a very ſmall part of an irregular object can be ſeen diſtinctly this way.

To

To remedy these defects, Dr. *Barker* has contriv'd a microscope on the model of the *Newtonian* telescope, in which he has been very much assisted by Mr. *Scarlet* Junior. As to the effects of this instrument, it magnifies from the distance of 9 to 24 inches.

Fig. 3. Plate VII. represents the entire microscope mounted on its pedestal, on a proper joint, contriv'd so as to direct the instrument towards any object.

Fig. 4. The section of the instrument, in which A B is the larger concave metalline speculum; C D the lesser concave metalline speculum; E F a hollow brass screw to fasten in the first dioptrical glass, or plano convex glass; G H another screw fastening on the hollow cylinder E F I K (in which the dioptric glasses are contain'd) to the body of the microscope; I K a cap with a small perforation, serving as an aperture to the eye-glass, or second *lens* (convex on both sides) M L a long screw passing thro' the nuts P and V, serving to bring the small *speculum* to a proper distance from the larger; N Q a sliding piece mov'd by the screw, carrying the stem Q R, and little *speculum* C D; Y X a screw for the cap as represented at Fig. 5; that at Fig. 6. is to be screwed on the aperture I K.

Fig. 7. shews the construction of the microscope, in which *i* is an object supposed erect, from which rays falling on the *speculum a b*, will be reflected to the *focus k*, where they will form an inverted image, and being reflected by the small *speculum c d*, they will pass thro' the perforation of the great *speculum*, and falling on the plano-convex glass *e f*, converge again, and form an erect image at *l*; which being brought very near to the eye, and so considerably magnified, will be distinctly seen thro' the eye-glass *g h*.

An Account of the standard Measures preserv'd in the Capitol *at* Rome; *by Mr.* Folkes. Phil. Trans. N° 442. p. 262.

IN the wall of the capitol is a fair stone of white marble, 8 foot 5 inches *English* in length, and 1 foot 9 inches and $\frac{1}{2}$ in breadth; upon which are inscribed the standards of several measures with the following respective inscriptions.

Piede Ro: Pal: IIII. Onc. XII. Deti XVI

Piede Greco.

Canna

PLATE VII.

Fig V.

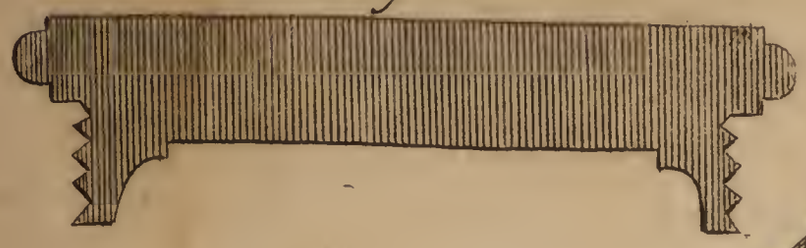
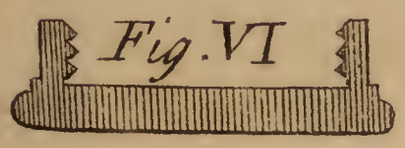


Fig. VI



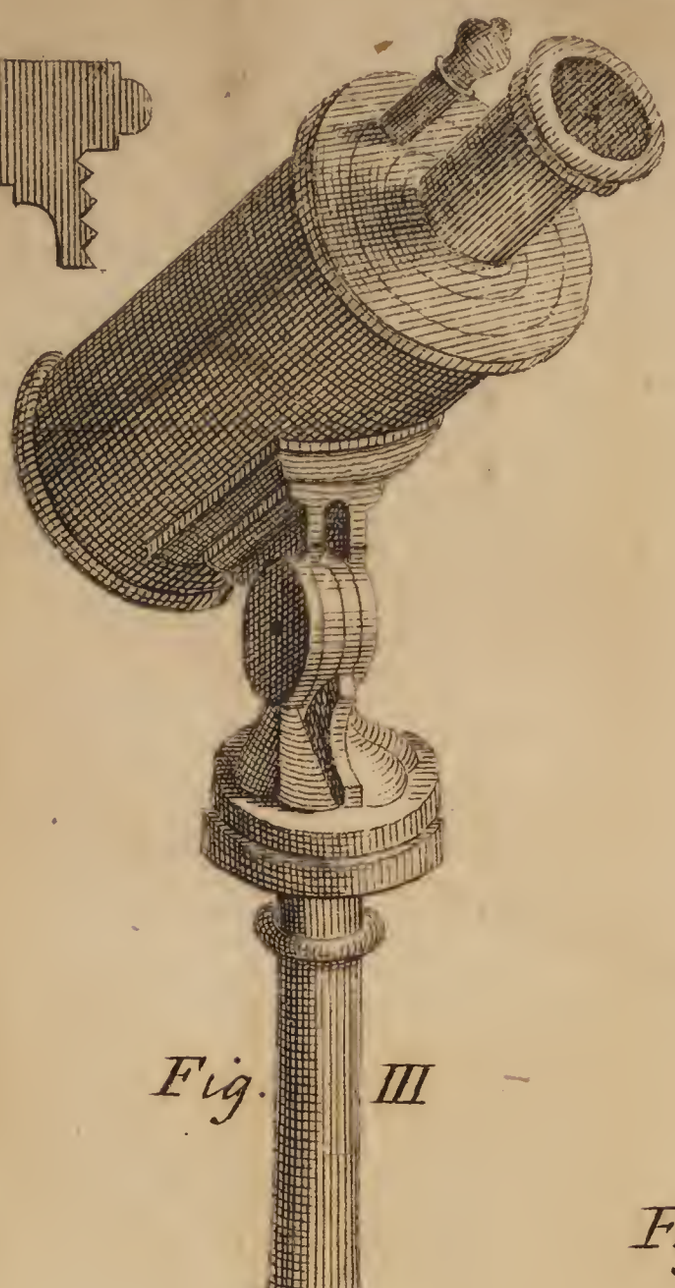
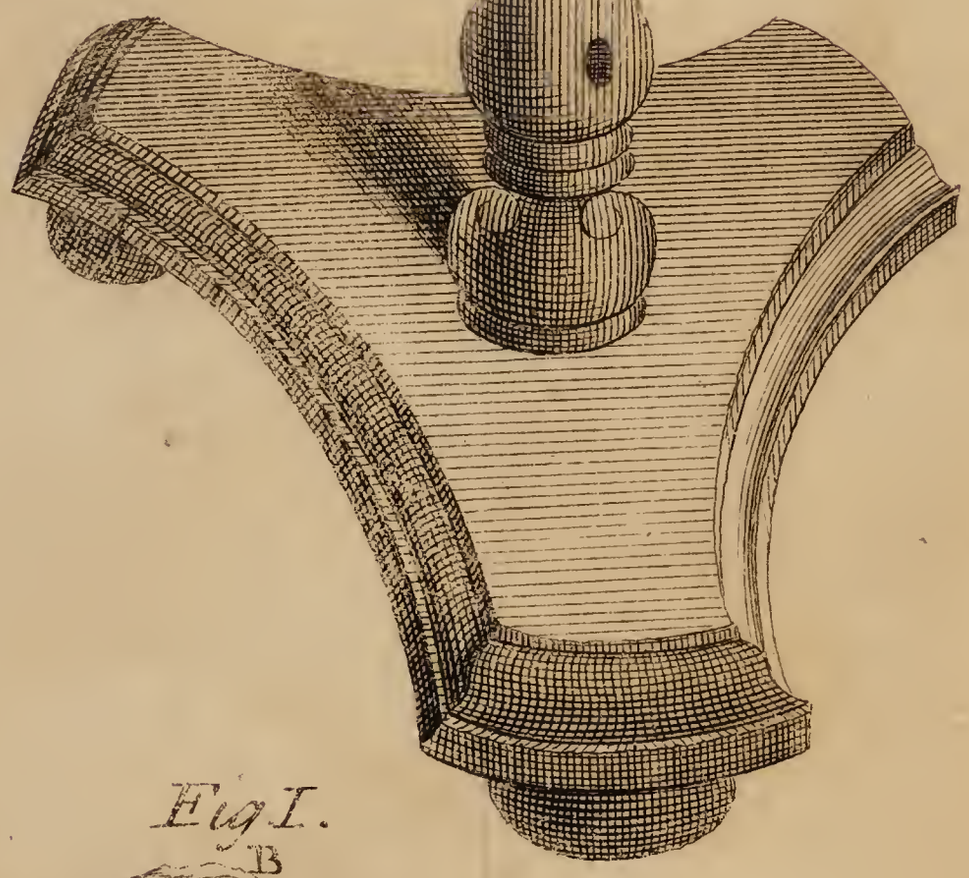


Fig. III

Fig. IV



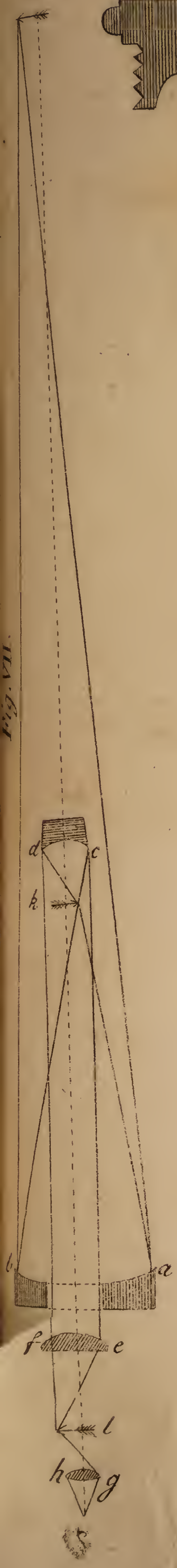


Fig I.

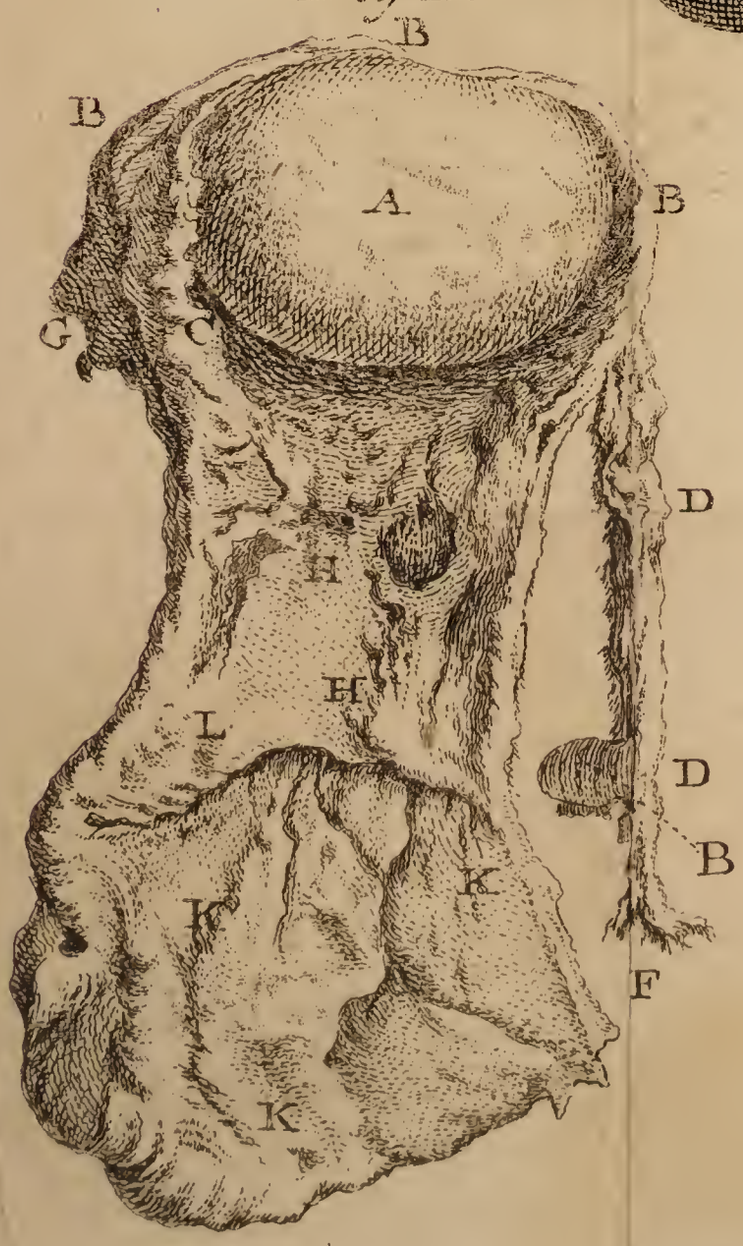
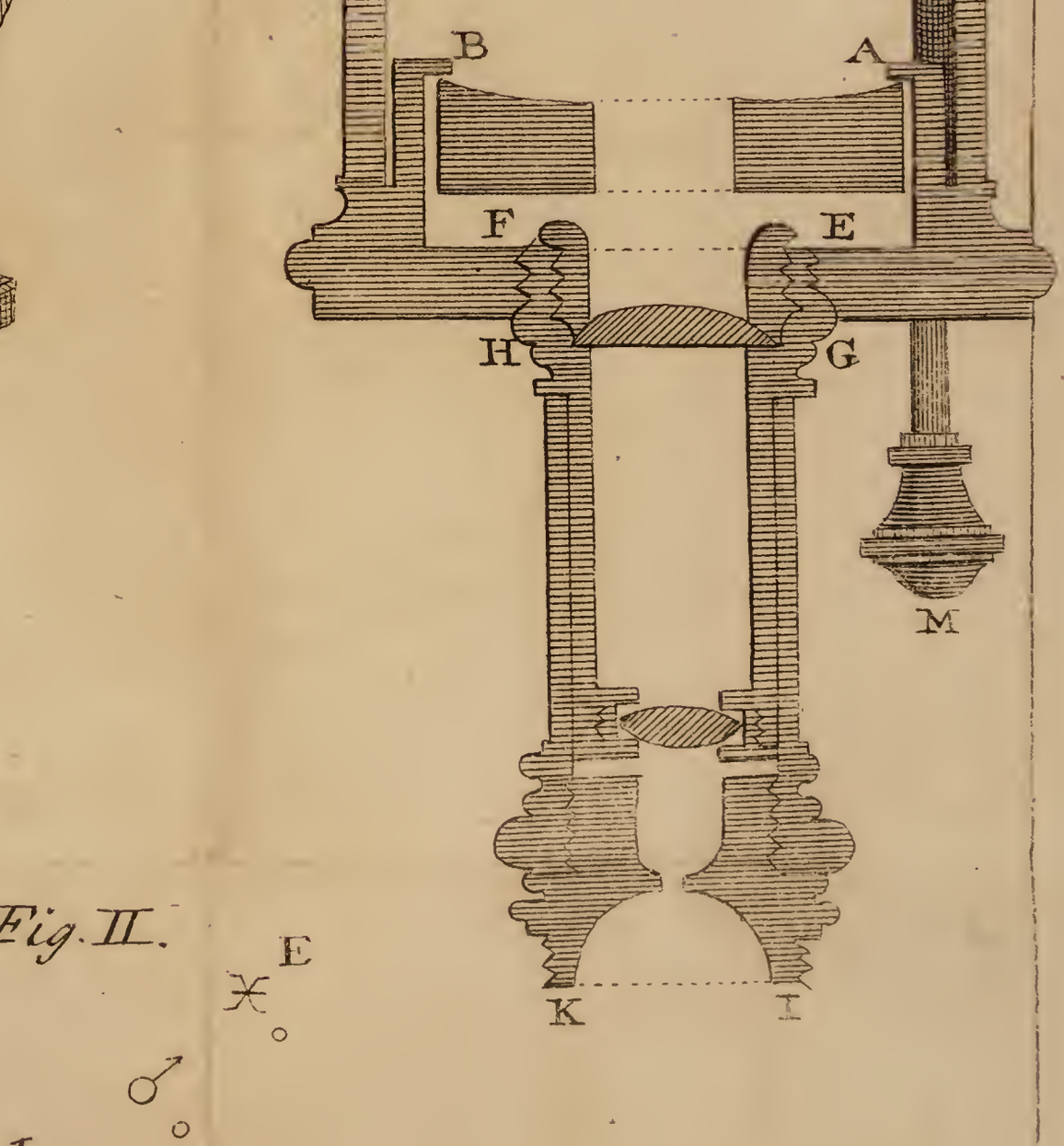


Fig. II.



Hulett Sculp.

nith delin!

Canna di Architet. Palmi X.
Staiolo Pal. V. Quar. III.
Canna di Merca. Palmi otto d'altra misura
Braccio di Merc. Pal. III. d'altra misura
Braccio di Tessitor di Tela

Curante Lu. Pœto

The lines, that represent these measures, are cut pretty deep in the marble; but as they have, consequently, a considerable thickness, it is somewhat difficult to be very exact in taking off their dimensions. Mr. *Folkes*, however, attempted to do it as near as possible, by setting the point of his compasses in the middle of the cross lines, that are drawn to determine the beginning and end of the measures. The palm of the architects is easier to give than the others, by reason the whole *Canna* is inscribed on the stone: This he, therefore, took off, as he presumes others have generally done, and then divided it into 10 equal parts. After this his chief attention was to the *Roman* foot, as being of greater consequence than the other measures. They all, however, follow as they occur'd to him, in such parts as the *London* foot contains a thousand.

The *Roman* foot 966 +. This is divided upon the stone; first into four palms, and then on the upper part into 12 unciæ; and on the lower into 16 deti, according to the inscription.

The *Greek* foot 1006 +. This is also divided like the *Roman*.

The *Canna* of the architects 7325. It is divided into 10 palms; each of which is, therefore, 732 and $\frac{1}{2}$ of the *English* foot.

The *Staiolo* being five palms and $\frac{3}{4}$ is 4212.

The *Canna di Mercanti*, divided into eight palms of another measure 6 foot 9 inches $\frac{21}{40}$.

The *Braccio di Mercanti*, divided into four palms of another measure, 2 foot 6 inches $\frac{11}{24}$.

The *Braccio di Tessitor di Tella*, divided into three parts 2 foot, 1 inch and $\frac{1}{2}$ 4.

The palm of the architects is assigned by Mr. *Greaves* 732 of the *English* foot; and the same is given by M. *Picart* to the *Paris* foot, in the proportion of 494 and $\frac{1}{4}$ to 720; which,

reduced becomes 732 ½ of the *Engliſh* foot, as before, and as it came out from Mr. *Folkes*'s own trial.

The *Roman* foot is given by M. *Picart* from this very ſtone: 653 $\frac{4}{10}$ of ſuch parts as the *Paris* foot contains 720; that is, by reduction 967 ½ of the *Engliſh* foot; and the ſame by *Fabretti*, who alſo meaſured it upon this ſtone, is aſſigned to the palm of the architects, as 2040 to 1545; which reduced upon the former meaſure of the palm is 966 ½ of the *Engliſh* foot.

Theſe meaſures come out as near as the nature of the ſtandard can poſſibly allow; and as it was ſomewhat freſher in M. *Picart*'s time than it is now, Mr. *Folkes* would not vary from the proportion the former has aſſigned, but ſuppoſes the *Roman* foot on this marble was intended to be ſuch a one, as ſhould contain 967 parts of the *Engliſh* foot very nearly.

Mr. *Greaves* had long before aſſigned the meaſure of the *Roman* foot from *Coſſutius*'s monument to be 967 of the *Engliſh* foot, and preferr'd that meaſure to the others he had taken from the tomb of *Statilius*, and the *Congius* of *Veſpaſian*. And Mr. *Folkes* thinks one can make no doubt, from what has been ſaid, but *Coſſutius*'s foot was that intended to be inſcribed upon this marble; tho' that monument is itſelf now loſt: At leaſt when Mr. *Folkes* was at *Rome* he could get no intelligence of it.

Fabretti in his work concerning aqueducts, where he gives the above mentioned proportion of the palm to the foot, finds fault with *Lucas Pœtus*, as having made a wrong calculation of this proportion in his book *de menſuris & ponderibus*. The proportion there given by *Pœtus*, does not, it is true, agree with the foot upon the marble; but yet it is no falſe calculation, as *Fabretti* thought; and had he carefully examined *Pœtus*'s book, he would have been ſenſible this is not the foot he there contends for, but the *Coſſutian* foot, which *Lucas Pœtus* in his book diſputes againſt. The truth, therefore, is, that he either alter'd his mind after the writing of that book, before the marble was ſet up; or more probably, that tho' he had the care of having theſe meaſures inſcribed on the marble, he was directed by a ſuperior authority what meaſures he was to have engrav'd; and that accordingly he had, as near as poſſible, the *Coſſutian* foot deſcribed for the ancient *Roman* foot on the ſtone: And that this was the caſe, and no miſtake about the number, as *Fabretti* ſuppoſes, appears not only from the tenure of his book, where he condemns *Coſſutius*'s foot, which

there

there appears, but also from his scheme at the latter end, where he has given what he calls *schema pedis legitimi*, agreeing with his own numbers, *viz.* 12 inches, whereof $9\frac{2}{5}$ make the palm of the architects and likewise the measure of the *Colotian* and *Statilian* foot, agreeing with that inscribed on the marble. The *Colotian* is the same monument as the *Cossutian*; so call'd from the person, in whose possession it had formerly been: And he had before said, p. 5. that according to the testimony of *Philander* the *Statilian* agreed with it, tho' Mr. *Greaves*, who measured both these feet very carefully, found some difference between them, stating the *Cossutian*, as above, 967, and the *Statilian* 972. But by *Pætus*'s quoting *Philander*, it is plain, he had not himself measured the latter: And therefore, the foot call'd by him the *Colitian* and *Statilian* is indeed purely the *Colotian*, or *Cossutian* foot; and the same has also occurred to Mr. *Folkes* very nearly from his measure of the height of the *Trajan* pillar, which he finds, from the ground to the top of the cimatium of the capitol, to be 115 foot 10 inches $\frac{5}{8}$; and this height, divided by 120, gives 966 for the quotient very nearly.

As to the *Greek* foot there seems to be no farther mystery, than that it was intended to be made to the *Roman* in the proportion collected from *Pliny*, which is, that 625 *Roman* feet made 600 *Greek*; by which account the *Greek* foot should contain 1007 of such parts as the *Roman* contains 967; and the actual quantity Mr. *Folkes* took off was 1006.

Some Observations in 1734, *at* Wittemberg; *by* M. Weidler. Phil. Trans. N° 442. p. 266. *Translated from the* Latin.

ON the 23d of *January*, 1734, at 7h. 6m. in the evening there again appeared an exceeding bright *aurora borealis*. Under the north was seen a black arch with a double white *fascia* resting on it, distinguish'd by a black middle tract; the last white arch rose 25 degrees high; in the black tract at times were kindled shining pyramids; from the reflected light near the zenith a similar white arch was produced. At 7h 30′ the white tract was diffused a pretty way beyond the west point, and its middle reach'd 50 degrees high; at 7h 35′ the shining tract reach'd the *vertex*, discontinu'd clouds here and there tended towards the west, a circumstance which seldom happens in *auroræ boreales*. M. *Weidler* was likewise surprized at some clouds, which stood about the north-east, and moved before the shining tract, beyond which clouds the light

of the *aurora* was distinctly seen: Whence it appears, that the light of the *aurora* reach'd far beyond the clouds. At 7h 38′ the shining tract of the *aurora* was descending below the horizon, especially towards the west: This likewise deserves our attention, namely, that tho' this *aurora* was exceeding bright, yet it emitted few shining pyramids. At 8h 30′ almost the whole light was set below the horizon; yett still near the west it was 10 degrees high; the tract of the black arch, together with the small white impending *fascia* was still seen at 10 o'clock under the north.

On the 27th of *January* at 6h. 23′ in the evening, there was observ'd a conjunction of the moon and *Venus*. M. *Weidler* found the angle of *Venus* 35′ of a degree. *Venus* regarded the south, and the Moon the north; at 6h 57′ the angle of *Venus* was = 26′; and in this observation a line drawn thro' *Venus* and both the cusps of the moon was a right one; afterwards the moon gradually elongated from *Venus*.

On the 17th of *February* the lowness of the mercury in the barometer was remarkable, being at 28 inches 7 lines, such as M. *Weidler* never before observ'd in these parts; from this might be presaged that terrible storm of wind, that seem'd to overturn every thing in its way; at three in the afternoon great numbers of tiles were blown off the houses; so that none could walk the streets with safety. In *Saxony* and other places of *Germany* this storm did a great deal of damage to the houses, woods, and gardens, pull'd up by the roots an incredible number of trees, broke down old oaks and other trees; and men in the open fields could not stand on their legs.

On the 19th of *March*, at 6h 45′ in the evening about the N. N. W. there appeared a black arch, lined with a broad white *fascia*; whose light was brighter towards the west, than towards the east, but without such shining pyramids or waves, as usually accompany *auroræ boreales*: There are, therefore, some *auroræ boreales*, that only shew a white tract under the north, but without any motion of the light.

On the 29th of *March* there appeared a remarkable *aurora borealis*, which M. *Weidler* first observ'd at 9 o'clock. The moon was inclining to the west, and only shone through the clouds; almost the whole heavens were overcast; and yet after the clouds divided, there was a shining tract about the north point, from which at times shot shining pyramids, an infallible sign of an *aurora borealis*: He likewise found behind

behind the clouds a ſhining tract of the *aurora* become an arch. The ſhining pyramids might be ſeen till 11 o'clock; an uncommon ſight to obſerve an *aurora borealis* when the sky is cloudy, and a wind blows, whereas at other times it commonly appears only when the sky is ſerene and calm.

On the 9th of *Auguſt* as M. *Weidler* was to obſerve *Saturn*, at 11 o'clock, ſome thin clouds that hung about the N. N. E. and N. b. E. ſeemed to be on fire all of a ſudden. But after emitting a few radiations towards the vertex, the burning ceaſed, and little clouds were driven by a gentle wind towards the eaſt.

From theſe and other obſervations it plainly appears, how variable a phenomenon the light of the *aurora borealis* is.

OB-

OBSERVATIONS *made on the* Latitude, *variation of the* Needle *and* Weather, *in a Voyage from* London *to* Hudson's-Bay, *Ann.* 1735; *by Cap.* Chriſtopher Middleton. *Phil. Tranſ.* No. 442. p. 270.

Month.	Day.	Hour.	Altit. of the Thermometer.	Altitude of the Barometer.	Lat. by Account.	Longitude from *Lond.* Acc.	Variat of the Compaſs	Lat. obſerv. by *Smith's* Quad.	Lat. obſerv by *Hadley's* Quad.	Latit. obſerv'd by *Elton's* Quad.	Latit. by a Sextant of *Ward & Smith*	Winds.	Remarks.
			° ′	° ′	°	° ′		° ′	° ′				
		6	27	29. 9			W.					S W	Variable, with ſome rain.
May	31	12	24	29. 6	60. 0	19.11	23					to	
		9	29	29. 7								S E	
		6	29. 7	29. 7								S E	Variable the firſt two parts, moderate the latter, freſh gales and a great ſea.
June	1	12	25	29. 5	59.50	23.33	24					to	
		9	25	29. 5								S W	
		6	26	24. 4								S W	
	2	12	26	29. 3	59.55	25.53	25	59.54	59.59			to	Cloudy and ſqually with rain.
		9	27	29 8								S by E	
		6	28	29. 9									
	3	12	26	28. 8	60	28. 3	26	59.57	60			S W	Frequent ſhowers of rain, with ſqualls.
		9	27	30. 1									
		6	29	30. 5									
	15	12	26	28. 8	28.25	60.40	27	60.39	6 .41			S W	Uncertain ſqually weather with a weſtern ſwell.
		9	27	30. 1									

	6	28	30								SW	Squally, with ſome rain
June 5	12	26	29. 8	60.31	32.12	27						
	9	25	29. 7									
	6	28	29. 7								SW to NNW	Variable, with ſqualls of rain.
6	12	25	29. 5	58.44	35 46	27	58.47	58.50				
	9	24	29. 5									
	6	28	29. 5								NNW to WNW	Variable freſh gales, with rain and ſqualls.
7	12	28	29. 9	58.11	36.58	28	58. 8	58.11				
	9	30	29. 8									
	6	30	29. 9								NNW to W by S	Moderate and fair weather with clouds.
8	12	28	29. 8	57.50	38.33	28	57.53	57.57				
	9	26	29. 6									
	6	26	29. 9								W b S to S b E	Rain and freſh gales.
9	12	25	28. 9	58.28	42.16	28	58.28	58.38				
	9	26	29. 6									
	6	31	29. 8								NW	Hard gales, with a great ſea.
10	12	28	30.	58.25	43.11	29	58.16	58.22				
	9	26	30. 4									
	6	30	29. 8								NW to NE	Moderate, with ſome rain.
11	12	27	29. 8	58.48	44.16	29	58.40	58.45				
	9	28	29 9									
	6	30	30								NNE to W b S	At 6 made Cape *Farewell*, from N b W to N b E 14 or 15 leagues diſtance; moderate with ſome rain.
12	12	28	29. 6	58.22	45.46	29						
	9	28	29. 8									

Month.	Day.	Hour	Altitude of the Thermometer.	Alt. of the Barometer.	Lat. by Account.	Longitude from *London* by Act.	Variation of the Compass	Lat. observ. by *Smith*'s quad.	Lat: observ'd by *Hadley*'s quadra.	Latitu: observ: by *Elton*'s quadra.	Latitu. by a sextant of *Ward* & *Smith*	Winds.	Remarks
			° ′	° ′	° ′	° ′		° ′	° ′	° ′			
		6	27	29									
June	13	12	27	29. 7	58.12	45.37	29	58.10	58.13			N W	Much ice in sight. An hard gale, with a great sea.
		9	32	29. 9									
		6	31	30. 1									
	14	12	26	29. 9	58.12	46.11	30	58.10	58.11			N N W to E S E	Most part hard gales. Under main sail and mizen.
		9	29	30.23									
		6	29	30. 5									
	15	12	29	30. 1	59.7	49.49	31					E S E to S E	Saw a large isle of ice. Pleasant weather.
		9	27	30									
		6	26	30. 1									
	16	12	27	30. 1	60.14	55. 3	33					S E b S	Fresh gales, and cloudy with rain.
		9	27	29. 9									
		6	30	31								S b E	
	17	12	26	29 9	61. 7	59.43	36					to S E b S	Moderate, but hazy, with rain.
		9	26	29. 9									
		6	31	29. 9									
	18	12	28	29. 5	61.23	63. 7	39					S E	Hazy, with rain and fogs; several pieces of ice.
		9	27	29 7									
		6	27	29.85								S E	
	19	12	31	30	61.47	63.16	39	61.44	61.49	61.54		S S W	Foggy, with calms. Much ice all round *Resolution*.
		9	30	29. 9								S b E	

June 20	12	29	30	61.40	64. 8	39			61.46		Wt S W b S	Moderate weather, moſt part foggy.
	9	30	29. 8									
	6	29	29. 8								Wt	Much ice all round; fore part foggy, latter clear.
21	12	29	30. 6	61	64.34	39	61. 4	61. 5	61.12		W N W	
	9	29	30. 8								N b W	
	6	32	29									The moſt part foggy and hazy weather.
22	12	27	29. 4	61.11	65.40	40		61	61.12	61.12	N b W	
	9	28	29. 9									
	6	30	29. 9								So	Fair, with much ſhatter'd ice, and large iſles.
23	12	27	29. 8	61 30	66.50	40			61.30	61.30	S W	
	9											
	6											Eaſy breezes, fair and clear, heavy ice all round.
24	12	24	29. 8	61.37	67.19	41	61.36		61.38		W N W	
	9	26	29. 6									
	6	28. 5	29. 9									
25	12	27	29. 6	61.53	68.12	42					Ditto	Hazy middle part, clear the latter.
	9	28	29. 8									
	6	28	29. 7									
26	12	27	29. 8	61.41	68.42	41					S W b W	Fair pleaſant weather; much ice.
	9	30	39. 9									
	6										N N E	Little winds, and fair pleaſant weather.
27	12	29	29. 7	61.30	68 42	41					W S W	
	9	29	29. 6									

Month.	Day.	Hour.	Altitude of the Thermometer.	Alt. of the Barometer.	Lat. by Account.	Longitude from *London* by Act.	Variation of the Compass.	Lat. observ. by *Smith*'s quad.	Lat. observ'd by *Hadley*'s quadra.	Latitu. observ. by *Elton*'s quadra.	Latitu. by a sextant of *Ward* & *Smith*	Winds.	Remarks.
			° ′	° ′	° ′	° ′		° ′	° ′				
		6	31	29. 8									
June	28	12	29	29. 6	61.33	69. 2	40						Moſt part foggy, with calms.
		9	29	29 75									
		6	28	29. 5									
	29	12	27	29. 6	61.30	69.13	40	61.30		61.30	61.20	ESE	Several large iſles of ice; foggy.
		9	25	29.75									
		6	26	29. 9								ESE	Little winds and calm; heavy ice all round.
	30	12	26. 5	29. 5	61.33	69.26	40					to	
		9	27	19. 9								WSW	
		6	29	29. 9									Faſt in thick ice, with fogs and rain.
July	1	12	27. 5	29. 7	61.28	70.10	41					ESE	
		9	27. 5	29. 6									
		6	32	29. 1								SE	Theſe 24 hours the firſt and latter parts freſh gales, middle a Storm of wind and rain.
	2	12	31	29	61.28	70.37	41					to	
		9	26. 5	29. 3								NE	
		6	29. 5	29. 5									
	3	12	28	29. 8	61.28	70.22	41					NW	Hard gales, with much ſnow.
		9	27. 5	29. 9									
		6	32	29. 9								NW	Moderate and fair weather, with much ice.
	4	12	29. 5	29. 6	61.28	70.26	40						
		9	25. 5	29. 3								N b W	

		6	29	29. 8									
July	5	12	27	29. 9	61.39	70.36	40	61.33		61.44	61.43	W b S	Working to windward in ice, sometimes in a clear.
		9	29	29. 7									
		6	31	29. 9									
	6	12	27. 5	29. 7	61.46	70.54	40		61.54		61.53	W S W to N E b N	Moſt part fair and pleaſant weather, with much ice.
		9	26	29. 7									
		6	27	29. 4									
	7	12	27	29. 6	61.55	71.13	40					N E to N W b N	Foggy weather, cloſe ice all round.
		9	27	29. 9									
		6	29	29. 7									
	8	12	26	29 75	61.46	70.21	40					Ditto.	Moderate, with fogs.
		9	25. 5	29. 9									
		6	29	29. 9									
	9	12	27	30	61.42	71.40	41			61.38		Ditto.	The firſt part hazy, the latter clear.
		9	27	30									
		6	32. 5	29. 7								N W to S E	Clear ſea, freſh gales and hazy.
	10	12	29. 5	29. 6	62.18	75.13	41						
		9	26	29.75									
		6	31	29. 7									
	11	12	27	29. 9	62.31	76	42					S b W	Foggy weather.
		9	28	29.75									
		6	31. 5	29. 9									
	12	12	31. 5	29. 9	63. 5	75.28						Ditto.	Ditto.
		9	28	30									

Month.	Day.	Hour.	Altitude of the Thermometer.	Alt. of the Barometer.	Lat. by Account.	Longitude from *London* by Act.	Variation of the Compass.	Lat. observ. by *Smith*'s quadra.	Lat. observ. by *Hadley*'s quadra.	Lat. observ. by *Elton*'s quadra.	Lat. by a sextant of *Ward* & *Smith*	Winds	Remarks.
			° ′	° ′	° ′	° ′		′	° ′	° ′	° ′		
		6	30	30. 2								S	Working in looſe ice; hazy, ſometimes clear.
July	13	12	29	30. 3	63.15	75				63.20		to	
		9	27. 5	29. 9								N W	
		6	31	30								N W	Much ice, fair and clear, with calms.
	14	12	27	30	63.25	74.38						to	
		9	25	30								Weſt	
		6	30	29. 9								S S E	
	15	12	27	29. 8	63.20	76						to	Much ice, freſh gales and hazy.
		9	27	29. 8								E S E	
		6	27	29. 8									Looſe ice, large clears in ſight of *Salisbury*.
	16	12	26	29. 7	63.10	77.20						S E	
		9	26. 5	29 7									
		6	27. 5	29. 7									
	17	12	27. 5	29. 6	63.10	78.20	44					E S E	Foggy, much ice and freſh gales.
		9	26	29. 7									
		6	29	29. 6									Looſe ice, eaſt end of *Nottingham*.
	18	12	26	29. 8	63	78	43					Calm.	N b E ſeven leagues
		9	26	29. 8									
		6	25	29. 9									Much ice, clear weather.
	19	12	24	29.85	63.10	78.20	43					E S E	Cape *Walſingham* S W b W five
		9	27	29.80									or ſix Leagues.

July

	6	28	29.85								SW	Shatter'd ice, foggy weſt; iſland *Diggs*, W S W five leagues.
July 20	12	27. 5	29.84	63.20	78.30	41						
	9	27	29. 9									
	6	27. 5	29.98								SSE	Cape *Diggs* S E b S 6 leagues. Freſh gales, and ice.
21	12	25. 5	29. 9	63.10	79.50	42						
	9	24	29. 8								S b W	
	6	27	29. 9								SW b W	Much large ice; the north end of *Mansfields* S W b W four miles.
22	12	25	29. 9	63	80.13	42					to	
	9	23. 5	30. 1								South.	
	6	26. 5	30. 1									North end of *Mansfields* S W four-
23	12	26	30	63. 4	81.10	44			62.52		SW	teen miles.
	9	25	30									
	6	29. 5	29. 9								E b S	
24	12	29. 5	29. 9	61.34	82.52	40			61.26		to	Hazy; ſome pieces of ice.
	9	26	29.85								SSE	
	6	31	29.82								SSW	
25	12	28	29.82	61.15	83 56	38			61.14	61.17		Much ice and rain.
	9	26	29.83								S b E	
	6	32	29. 7								S b E	Thick foggy weather, with ſhowers of rain.
26	12	29	29. 7	60.45	84.23				60.41		to	
	9	29. 5	29 9								W b N	
	6	30. 5	29. 9								NW	
27	12	26	29. 9	59.39	84.11	30	59.36		59.34		to	Steering ice, with wet fog.
	9	26	30 1								Weſt.	

Month.	Day.	Hour	Altitude of the Thermometer.	Alt. of the Barometer.	Lat. by Account.	Longitude from *London* by Act.	Variation of the Compass	Lat. observ. by *Smith*'s quad.	Lat. observ'd by *Hadley*'s quadra.	Latitu. observ. by *Elton*'s quadra.	Latitu. by a sextant of *Ward* & *Smith*	Winds.	Remarks
			° ′	° ′	° ′	° ′		° ′	° ′	° ′			
		6	28	30. 1									*Baker's Dozens* S b W four leagues.
July	28	12	26	30. 1	58. 9	82.44	28	58. 5	58. 2			N b W	Fair weather.
		9	26	30 1									
		6	28	30.									Moderate, with fogs, rain and ice.
	29	12	26	30.	56.46	82.45	26			56.30		W b S	
		9	25	29.95									
		6	27. 5	29.85									Freſh Gales. A great ſea from the
	30	12	25. 5	29. 9	55.51	82.39	25					N W b W	ſouthward.
		9	23	30									
		6	26	29. 9									Thunder and rain. *North-Bear*
	31	12	25. 5	29. 5	54.44	82.47	24					N E	S W by W five miles.
		9	24	30									
		6	26	30									
Aug	1	12	25. 5	29.98	53. 3	81	22					N E	Moderate and fair weather.
		9	23	29.85									
		6	24	29. 8								South	The firſt part moderate, the latter
	2	12	23. 5	29. 9	52.20	82						to	hard gales, with thunder and light-
		9	24	29. 9								North	ning in with the W. main.
		6											
	3	12	26	30	51.40	83							Arrived in *Mooſe-river* road.
		9											

											Winds.	
Aug. 4	6											
	12											
	9											
					West						West	
Sept. 1	12	27	29.85	55.36	00 24	22					and	Moderate and fair.
											W b N	
	9	29	29.85		from						N W	Moderate gales and hazy, with
2	12	27	30. 5	53.50	*Bear.*	26					to	small rain.
	9	26	30. 8		1.16						North.	
	9	⊙	29. 8								N b W	
3	12	26	30. 4	56. 6	1.16	26					to	Ditto.
	9	23	31. 5								N b E	
	9	30	29. 8								South	The firſt part moderate, middle and
4	12	26	30. 6	58. 6	2 58	30					to	latter very hard gales, with
	9	23	31.								Eaſt	ſqualls of rain.
	9	33	29 8									
5	12	33	29.85	58.28	4.26	30					N E b E	Ditto, hard gales and ſqualls.
	9	34	30.									
	9	33	30. 1								E N E	Freſh gales and ſqually, with a
6	12	32. 5	30. 2	58.40	3.23	31	58.42	58 49			North	head Sea.
	9	32	30. 2									
	9	27	29. 9									
7	12	27	29. 8	60.45	1.15	31					N W	Ditto gales and ſnow
	9	26	30. 1									

Month.	Day.	Hour.	Altitude of the Thermometer.	Alt. of the Barometer.	Lat. by Account.	Longitude from *London* by Act.	Variation of the Compass.	Lat. observ. by *Smith's* quad.	Lat. observ'd by *Hadley's* qnadra.	Latitu. observ. by *Elton's* quad.	Latitu. by a sextant of *Ward* & *Smith*.	Winds.	Remarks.
			°	° ′	°	°	°	°	°	°	°		
		9	37· 5	30. 3		East.						S W	Strong gales and squally ; great sea
Sept.	8	12	35	30. 2	61.37	0. 4	32					to	from the north weſt.
		9	32	30. 1								N W	
		9	34	30. 3		East						N W	Moderate and fair ; four leagues
	9	12	32 5	30. 3	61.55	0. 4	33					to	from the ſouth end of *Mansfields*
		9	32	30. 5								N N E	76 Fathom, Mud.
		9	32	30. 5		Weſt.						N b E	Little Winds and fair Weather.
	10	12	29	30. 5	61.58	1. 0	34	61.43	61. 45	61.46			In ſight of *Mansfields*
		9	30	30. 5								S W	
		9	36	30.25		East.							Fair and pleaſant weather.
Oct.	11	12	30	30. 1	62.30	2.21	43	61.30				S W	In ſight of *Mansfields*, 4 Miles.
		9	28	30.									
		9	30	30. 1		East.						W S W	At noon *Mansfields* N W b W 5
	12	12	30	30.	62.52	2.28	42					to	Leagues ; fleet and foggy. Cape
		9	30	30. 1		2.28						W N W	*Walſingham* S W 6 leagues.
		9	32	30. 3		fro. *Dig*						N W	Frequent ſhowers of hail and ſnow
	13	12	33	30.25	63, 9	3.40	43					to	
		9	32	30. 2		East.						N b E	
		9	31 5	30. 1		East.						N N E	
	14	12	29	30. 5	62.50	6. 4	42						Moderate and fair weather.
		9	31	30.								Calm.	

		9	30. 5	29. 5							to	
Sep.	15	12	30.	29.46	62.22	7.55	42	62.21	62.23		W N W	Much ſnow, with thick weather
		9	29.	29. 5								
		9	29	29. 8							N W to N b E	Paſs'd ſeveral large iſles of ice; freſh gales and fleet: thick weather with ſnow.
	16	12	30	30	61.43	12	42					
		9	28	30. 3								
		9	32. 5	30. 4							North to N N E	First two parts hard gales and ſnow, latter moderate. At noon cape *Reſolut.* NE 6 L.S.Pt.NW. bN4L.
	17	12	33	30. 5	61.43	15.10	40					
		9	31	30.55								
		9	31	30. 4		West						Many large iſles of ice; hard gales; great ſea from the north eaſt.
	18	12	34	30. 3	60.40	58.40	40				N E b N	
		9	31	30. 4								
		9	33	29. 9							North to S W	Moderate, but dark and cloudy, with fleet and rain.
	19	12	26	29. 5	59.49	53.43	38					
		9	26	29. 5								
		9	30	29. 7							N W	Freſh gales, with rain and ſnow
	20	12	29	29.65	58.42	48.52	32	58.37	58.38			
		9	29	29. 6								
		9	31	29. 5							N N W to North	Variable, with freſh gales and rain
	21	12	30	29. 6	57.28	44.31	29					
		9	29	27. 7								
		9	27. 5	29. 5							North to E S E	Freſh gales and ſqualls; variable winds and cloudy.
	22	12	27. 5	29. 5	56.49	41.39	27					
		9	29. 6	27. 9								

Month.	Day.	Hour.	Altit. of the Thermometer.	Altitude of the Barometer.	Lat. by Account.	Longitude from *Lond.* Acc.	Variat of the Compass	Lat. observ. by *Smith's* Quad.	Lat. observ by *Hadley's* Quad.	Latit. observ'd by *Elton's* Quad.	Latit. by a Sextant of *Ward & Smith*	Winds.	Remarks.
			° ′	° ′	° ′	° ′		° ′	° ′	° ′			
		9	29	29·65								NEbE	
Sep.	23	12	26 5	29· 5	55·52	36.50	25	55·44	55·47			to	Hard gales with rain and fogs
		9	26	29. 7								NbW	
		9	31	29· 8									
	24	12	27	39· 9	55· 3	31.21	24	55· 1	55· 4			NNW	Freſh gales and rain
		9	26	29· 8									
		9	27· 5	30								NWbN	Hard gales, with ſqualls and ſmall
	25	12	26	30	53·43	25·59	23					to	rain.
		9	26	30 2								SWbW	
		9	21	30. 2								SW	Firſt part hard gales, middle and
	26	12	19	30 2	52.38	22.23	22					to	latter moderate and hazy.
		9	20	30. 1								SWbS	
		9	21	30								South	Freſh gales and much rain; hazy
	27	12	20. 5	31	52.27	20.15	21					to	weather
		9	24	30. 1								NNE	
		9	25	30. 2									
	28	12	25	30.25	50.30	16.33	20	51	51			NE	Freſh gales, and much rain; hazy
		9	24	30 2									weather
		9	23	30. 3								NE	Freſh gales and hazy, with ſome
	29	12	23	30. 2	50.20	13. 7	19	50.24	50.25			to	rain the firſt two parts, the latter
		9	24	30. 2								NbW	moderate

		9	23		30. 2								
	30	12	21.	5	30. 3	50.37	11.41	18	50.30	50.35			to S by E	Moderate and fair; winds variable.
		9	20		30. 2								
		9	22.	5	30. 3								
Oct.	1	12	22		30. 2	50.35	11.22	18					S S E	Moderate, with a great head-sea from the eastward.
		9	22		30. 3								
		9	22		30. 3									
	2	12	23		30. 2	49.40	12.32	18					S E by S	Cloudy, with a great sea from the eastward.
		9	22		30									
		9	22		29								S S E	Hard storm of wind, with a great sea.
	3	12	22.	5	29	50. 7	11.39	18					to S by E	
		9	22.	5	30. 1									
		9	20		29. 9								S S W	Moderate gales, with small rain.
	4	12	20		30. 1	50. 3	10.36	17	50. 4					
		9	20		30.25									
		9	20.	5	30.35								S S W	
	5	12	20.	5	30. 3	49.38	8.52	16	49.42				to West	Ditto, fair and moderate.
		9	20.	5	30. 4									
		9	22		30. 2								South Calm	Saw several Ships, sounded 100 fathom. Moderate.
	6	12	22		30. 1	50	7.20	15	50. 5				S S E.	
		9	22		29. 9									
		9	22		29. 9								S S E	Two first parts hard gales and heavy squalls, the latter moderate.
	7	12	21		30	49.39	6.58	15	49.38					
		9	21		29. 9								W N W	

Explanation of the Tables.

The firſt column contains the month; the ſecond, the day of the month; the third, the hour of the day, beginning at ſix in the morning to twelve at noon, and nine at night; the fourth column contains obſervations of the thermometer; the fifth, the height of the mercury in the barometer, where the firſt number is the height in inches, the ſecond and third numbers mark the tenths and hundredth parts of an inch; the ſixth column is the latitude the ſhip is in by account, every day at noon; the ſeventh column is the longitude the ſhip is in every day at noon, by account, from the meridian of *London*, except where otherwiſe expreſſed; the eighth column is the variation of the needle; and the next four columns are the latitudes obſerved at noon by four ſeveral new inſtruments; the firſt is Mr. *Smith*'s priſmatic quadrant; the ſecond, Mr. *Hadley*'s; the third by Mr. *John Elton*; and the fourth by Mr. *Caleb Smith* and Mr. *William Ward*; the next column is the wind for the moſt part of the 24 hours.

The thermometer Capt. *Middleton* made uſe of in the voyage was made by Mr. *John Patrick*, as alſo the barometer: In his thermometer Mr. *Patrick* places 0 at the top, ſuppoſing it to be the heat under the line; and ſo the figures increaſe downwards with the increaſe of cold: Temperate is placed at 25.

Mr. *Smith*'s priſmatic quadrant Capt. *Middleton* finds to be of very great uſe at ſea, particularly for the ſtars, as he experienced ſeveral times in his voyage to *Hudſon's-Bay*, in the worſt of weather, when one can but ſee the horizon: And his other is of great uſe, in tolerable ſmooth water, in foggy and hazy weather, when there is no horizon to be ſeen, yet have the benefit of the ſun.

An experiment to ſhew that ſome damps in mines may be occaſioned only by the burning of candles under ground, without the addition of any noxious vapour, even when the bottom of the pit has a communication with the external air, unleſs the external air be forcibly driven in at the ſaid communication or pipe; by Dr. Deſaguliers. Phil. Tranſ. N°. 442. p. 281.

Exp. 1. IN a cylindric glaſs receiver, open at both ends, whoſe lower end is plunged in water, and upper end covered with a plate with a hole of near an inch bore, a candle

candle of ſix in the pound will not burn quite the time of one minute before it goes out.

Exp. 2. A candle will burn almoſt as long when the receiver is quite covered.

Exp. 3. The receiver having the hole of the plate open, and a pipe at bottom, communicating with the external air, will burn but a little longer than in the firſt experiment; and if you blow in at the pipe with your mouth, it will go out rather ſooner.

Exp. 4. Blow in at the pipe with bellows, and the candle will burn as long as you will.

A chemical experiment (ſerving to illuſtrate the phænomenon of the inflammable air, ſhewn to the Royal Society *by Sir* James Lowther) *by Mr.* John Maud. Phil. Tranſ. N°. 442. p. 282.

SIR *James Lowther* having made an experiment on ſome air which he collected out of a coal mine, and brought in bladders cloſe tied by ſea to town: Vid. *Phil. Tranſ. N°.* 429. *p.* 109. The effect of which was, that the air, being preſſed out of the bladder thro' the ſmall orifice of a tobacco-pipe, would catch fire from a lighted candle, and burn like an inflammable ſpirit, till it was all conſumed.

Upon conſidering that this was only owing to a great quantity of ſulphurous fumes floating in that air, Mr. *Maud* was induced to make an eſſay, by an artificial mixture, to produce the like effect. It is well known to every one converſant in chemical matters, that moſt metals emit great quantities of ſulphurous fumes, during the efferveſcence they undergo in their ſolutions in their reſpective *menſtrua*: Iron, whilſt diſſolving in oil of vitriol, emits a great quantity of theſe fumes, which are very inflammable, and not eaſily to be condenſed. Theſe fumes Mr. *Maud* collected into a bladder with the deſired ſucceſs, and having produced before the *Royal Rociety* two bladders of this factitious air, at the ſame time that Sir *James Lowther* made trial of his, they both exhibited the ſame phænomena. A more particular account of the preparation made uſe of is as follows.

Mr. *Maud* took two drachms of oil of vitriol, and mixed it with eight drachms of common water, which he put into a glaſs with a flat bottom, about ten inches wide, and three inches deep, with a long neck; to this he added two drachms of iron filings: There inſtantly aroſe a great heat, with a violent ebullition, and the iron was wrought upon very faſt, with a copious exha-

exhalation of fumes. To the end of the neck of the glaſs he luted a bladder void of air, the neck of the bladder being faſtened to a tobacco-pipe; the fumes ariſing from the diſſolving metal ſoon puffed up the bladder to its full extent, and after that another bladder in the ſame manner; and thus you may get as many bladders full as you can, whilſt the effervescence laſts. Two of theſe bladders were tried before the *Society*, and exhibited a flame very like that of Sir *James Lowther*'s in the ſmell, tho' ſomewhat different in the colour of the flame. After Mr. *Maud* had preſſed part of the air out of the bladder, by drawing back the hand, the flame was ſucked into the bladder, and all at once ſet on fire what inflammable air remained; which went off like a gun, with a great exploſion.

What is worthy of notice in this experiment is, that all the air, which filled the bladders, was generated *de novo*, as it were, out of the mixture, or elſe recovered from the body of the metal in an unelaſtic ſtate.

This experiment will eaſily explain a very probable cauſe of earthquakes, vulcano's, and all fiery eruptions from the earth: For, nothing more is requiſite than iron, a vitriolic acid and water: Now iron is generally found accompanied with ſulphur; and common ſulphur may be analyſed into an inflammable oil, and an acid liquor like oil of vitriol. This acid, therefore, in the bowels of the earth, by being diluted with a little water, ſurrounds the iron, and works upon it in the ſame manner as deſcribed above; an effervescence and inteſtine heat ariſes; the air, which comes from the mixture, is rarified, and becomes very elaſtic; its impetus, by how much the more compreſſed by the incumbent weight of earth, is increaſed even to an unlimited degree; and at length like gunpowder will remove all obſtacles, and exhibit to the ſpectators above ground the terrible phænomena of earthquakes and eruptions. Sometimes theſe inflammable fumes, if very much heated, will, as ſoon as they come to the open air, catch fire; and ſo produce thoſe fiery eruptions, of which there are ſo many inſtances in the world.

An account of the ſtorm, Jan. 8. 1734-5; *by Mr.* Forth. Phil. Tranſ. N°. 442. p. 285.

AT *Darlington*, 14 miles ſouth from *Durham*, Lat. 54°. 46', the evening before the 8th of *January*, 1734-5, Mr. *Forth*'s barometer ſtood at 29 inches, but had been gradually falling for two days. The wind was then ſouth weſt, and of the

the ſecond degree of ſtrength; and increaſed towards midnight a degree more. Moſt of the day there was ſnow or ſleet.

The 8th in the morning he found his glaſs fallen to 28 inches, 38 parts, and at four o'clock *p. m.* down to 28 inches, 5 parts; and by 10 in the evening riſen again to 28 inches, 45 parts. All this while the wind was in the north-eaſt, with only a moderate gale, tho' attended all day with ſnow, which at night was 2 inches and $\frac{1}{4}$ deep; and about eight o'clock it began to freeze. As the wind in the ſouthern parts was all that while in the oppoſite quarter, Mr. *Forth* would have expected an accumulation of the air, and as a conſequence thereof, the riſing of the barometer at the time of its falling the loweſt. Had the ſtorm happened the night before, when in the northern parts the wind was in the ſame direction, and had afterwards fallen, he would then have imputed the fall to the quick return of the current of air.

In the *December* of the preceding year Mr. *Forth* finds by his regiſter that there fell through his funnel, whoſe area is juſt 100 inches, 13lb. 85parts; yet their almoſt conſtant intermitting froſts kept it from going off in any conſiderable quantity at a time.

	Inches.
Greateſt height of the barometer,	30. 10
Leaſt ditto,	29 13

Of the bones of animals changed to a red colour by aliment only; by Mr. John Belchier. Phil. Tranſ. N°. 442. p. 287.

THAT the circulation of the blood is carried on through the bones, is evident from ſeveral phænomena obſervable in ſurgery: But that it is univerſally and intimately diſtributed thro' the moſt ſolid and compact ſubſtance of the bones (tho' hitherto doubted by ſome) will appear undeniably from the inſtances here produced; which are the bones of ſeveral hogs, of a different breed, changed to a deep red colour, merely by aliment. And what makes this ſtill more ſurpriſing is, that neither the fleſhy nor cartilaginous parts ſuffer the leaſt alteration in colour or taſte.

Their diet was bran, after boiling it in a copper with printed callicoes, in order to clean them from a dirty red colour occaſioned by an infuſion of madder root, made uſe of to fix the colours printed on the cloth: Some of theſe colours are made with preparations from iron, others with a mixture from alum and ſugar of lead. The parts printed with the

pre-

preparation of iron produce black and purple; thoſe printed with the mixture of alum, red of different degrees, according to the ſtrength of the mixture. The bran after abſorbing the red colour diſcharged from the cloth was mix'd with the common food of the hogs, and produced this effect on their bones.

Upon examining theſe bones, he obſerv'd in general the ſolid parts to be moſt tinged, and the teeth in particular, except the enamell'd part, which is of a different ſubſtance: And upon ſawing them through, he found the internal parts equally tinged, except at the ends of the bones, where the ſubſtance was more ſpongy: And in order to diſcharge the colour, he macerated them in water for many weeks together; boil'd them often, and ſteep'd them in ſpirits, but all prov'd ineffectual: nor was the leaſt tincture given to any of the liquids, in which he made experiments.

An Obſervation of white Liquor like Milk, that appeared inſtead of Serum, ſeparated from the Blood, after it had ſtood for ſome time; by Dr. Alexander Stuart. Phil. Tranſ. N° 442. p. 289.

ONE *John Wicks*, carver, in *Bromley-ſtreet*, *London*, about 40 years of age, was ill about three weeks by the loſs of appetite, and by indigeſtion, and at laſt a pain and diſtention of his ſtomach, with a low degree of an inflammatory fever; his tongue was dry, rough, and of a ruſty brown colour in the middle, with a white ſoft liſt on each ſide; his urine very high coloured, with a large quantity of a ſlimy pink-colour'd ſediment; his ſtools very yellow, and looſe.

After taking away eight ounces of blood, inſtead of ſerum there appeared nothing above the *coagulum*, but a white liquor like milk, which was pour'd off to the quantity of four ounces, or thereabouts. At firſt there was no ſmell perceptible, but in ſix days it began to have the ſmell of rotten eggs: For three weeks longer it ſtood in a room, where there was a fire for ſome hours of the day, and in that time it neither alter'd its conſiſtence nor ſmell.

The patient had eat very little for a week before the Dr. firſt ſaw him, and only a little of a calf's foot ſtew'd the night before for ſupper, and no breakfaſt that day. He was addicted to drinking of ſtrong pale-malt liquor every day he was in health.

If

If this be chyle, it is a ſubſtance very different from milk, which is apt to turn ſour and thick by keeping, and never contracts the putrid ſmell of rotten eggs, as this did. Whether it be not chyle turn'd putrid and near to purulency, by a long circulation in the blood-veſſels, but not converted into blood, through ſome defect in the ſanguification, is a queſtion the Dr. doubts cannot be decided without more obſervations and experience.

The *coagulum* of the blood was cover'd with a ſizy pellicle, about the thickneſs of a ſhilling: The red part was of a grumous, tender, incoherent conſiſtence.

Tho' the patient was much better in a week's time, the Dr. order'd five ounces of blood to be taken away, to ſee what change had happen'd; and he found rhe *coagulum* cover'd with a ſizy pellicle to the thickneſs of half a crown, the red part of a due conſiſtence, the ſerum clear and without any chyle.

The urine became clear, and he recover'd in about two weeks after the Dr. had firſt ſeen him.

An account of what was obſerv'd upon opening the body of a perſon who had taken internally ſeveral ounces of crude mercury; and of a plumb-ſtone, lodged in the coats of the rectum; by Dr. Madden. Phil. Tranſ. N° 442. p. 291.

THE internal uſe of crude mercury is of late become ſo frequent, that Dr. *Madden* believes it might hereafter produce ſome great benefit to mankind, if a careful collection were made of all the extraordinary caſes, relating to the good or bad effects of this practice.

In the following caſe there are circumſtances which were not owing to the internal uſe of mercury; yet there are many others which were undoubtedly occaſioned by it.

Dr. *Madden*, with Dr. *Robinſon*, and Mr. *Nichols*, ſurgeon-general, was preſent at opening the body of a gentleman of note at *Dublin*, who for ſeveral years found great difficulty in going to ſtool. This diſorder increaſed upon him towards the latter end of his life, and he was ſeized with a violent diſtemper, of which Dr. *Madden* could give no deſcription, having never attended him.

In order to procure a paſſage downward, (which the Dr. ſuppoſes was a principal complaint) the patient took, by the advice of a phyſician, ſeveral ounces of crude mercury, at different times, without any relief, and at length he died.

Upon opening the abdomen, which was very much diſtended, there burſt forth a great quantity of wind, tho' the gutts and ſtomach were not wounded.

The ſtomach was empty; and upon opening it the inner coat was found very much inflam'd from one end to the other. In ſeveral places of the ſmall guts were obſerv'd ſome ſcatter'd grains of crude mercury, and alongſt with them was generally found a black gritty powder, very like *Æthiops*, mineral which, without doubt, was the mercury chang'd into that conſiſtence.

The *colon* was diſtended at its origin, to twice the thickneſs of an ordinary man's arm about the ſhoulder. This extraordinary thickneſs extended itſelf about the length of 10 or 12 inches; from hence it gradually decreas'd; and where it was attached to the ſtomach, it had not above ½ of that ſize.

It was much inflam'd at its origin, and contain'd at leaſt ſix quarts of liquid excrement, in which was obſerved crude mercury, and likewiſe ſome of the black powder mentioned above.

The *colon*, where it parted from the ſtomach, and diverged towards the left kidney, adher'd about the ſpace of three inches to the *omentum*; and upon ſeparating it, there was found an abſceſs and inflammation, which had communicated itſelf to thoſe parts of the *ileum*, that were contiguous to the *colon*.

In this place the *colon* had a perforation about ¾ of an inch in diameters, and four ſmaller perforations, about the ſize of a gooſequill, thro' which ſome excrement had paſſed into the abdomen.

The coats of the *colon*, as it approached the *rectum*, began to grow ſcirrhous, about the ſpace of ſix inches, and the capacity became gradually ſmaller.

The valves of the *colon* about this place were of a reddiſh colour, and more ſcirrhous than the other parts of the inteſtine. The coats of the *colon*, where it was continued to the *rectum*, were at leaſt ½ an inch thick, and its capacity not above ¼ of the natural ſize.

Upon cutting the gut horizontally hereabouts, there was perceiv'd a body which ſtopped the paſſage, and ſeemed to the touch almoſt of a cartilaginous conſiſtence. Having open'd the gut lengthwiſe, it was found to be no other than 2 of the *valvulæ*

vulæ conniventes coli, grown ſcirrhous, and protruded downwards into the *rectum*.

There was alſo found a ſmall plumb-ſtone in this place, quite buried in the *tunica villoſa*, making itſelf a bed between the coats of the *rectum*: It had likewiſe formed a ſmall *abſceſs*, which diſcharged itſelf into the cavity of the *pelvis*, but had no communication with the cavity of the *rectum*.

A ſolar Eclipſe *obſerved at* Rome May 3, 1734. N. S. *by the* Abbè de Revillas, *and M.* Celſius. Phil. Tranſ. N° 442. *p.* 294. *Tranſlated from the* Latin.

THE *Abbè de Rivillas* and M. *Celſius* obſerv'd this ſolar eclipſe at *Rome* in the palace of Cardinal *De-via*; they took all poſſible care to determine the true time, both from the appulſe of the ſolar ſpecies to the meridian line, and from equal altitudes of the ſun, taken ſeveral times the ſame day, and on the day preceeding the eclipſe. They moreover obſerv'd the eclipſe with 2 very good teleſcopes, about 6 *Roman* palms in length: For, the too great altitude of the ſun, being almoſt in the meridian, and ſome other inconveniencies hinder'd their making uſe of longer ones: With the one, which tranſmitted the ſolar image directly to the eye, armed with a ſmoak'd glaſs, they principally obſerved the beginning and end of the eclipſe; and with the other, which exhibited, as uſual, the ſolar ſpecies, the intermediate phaſes. The clouds had like to have cut off all hopes of being able to make ſuch obſervations; as at ſun riſe they tinged the ſouthern parts of the horizon, and afterwards almoſt the whole heavens. But as thinner clouds ſucceeded thicker ones, tho' they deprived them of the firſt contact of the planets, and exhibited the firſt phaſes ſomewhat uncertain, and the ſpecies, about the end of the eclipſe, indented, as it were; yet they allowed accurately to determine the reſt of the obſervations.

Obſervations	True time p. h.	m. ′	″	quantity of obſcuration Dig.	
1	22	22	35	-------	The beginning ſeem'd thro'
2		27	1	0 ½	a cloud to have preceded
3		34	0	1	a little.
4		42	6	1 ½	
5	23	0	52	2	
6		3	16	2 -----	Or a little more; and the
7		10	31	2	greateſt obſcuration was
8		28	16	1 ½	thought to be at hand.
9		45	11	0 ½	
10		52	1	0 ------	The end.

From the 4th and 8th obſervations we may gather that the greateſt obſcuration happen'd about 23 h. 5 min.

The Deſcription and Manner of uſing an Inſtrument for meaſuring the Degrees of the expanſion of Metals *by Heat; by Mr.* John Ellicott. Phil. Tranſ. N° 443. p. 297.

AA (Fig 1. Plate VIII.) repreſents a flat plate of braſs, which, for farther ſtrength, is ſcrew'd down to a thick piece of mahogany: Upon this plate are ſcrew'd 3 pieces of braſs, two of which, markt BB, ſerve as ſupports for the flat iron bar C; and which, on account of its uſe, Mr. *Ellicott* calls the *ſtandard-bar*. The upper part of the 3d piece of braſs is a circle about 3 inches diameter, divided into 360 equal parts or degrees: Within this circle is a moveable plate, divided likewiſe into 360 parts, and a ſmall ſteel index: The braſs circle in the Fig. is marked D and the moveable plate *d*. Upon the ſtandard bar is laid the bar of metal, E, on which the experiment is to be made. F repreſents a lever 2 inches and ½ in length, faſten'd to an axis, which turns in 2 pieces of braſs ſcrew'd to one of the ſupports, markt B: To the end of this lever is faſten'd a chain, or ſilk line, which, after being wound round a ſmall cylinder, to which the index in the braſs circle D is faſten'd, paſſes over a pulley, and has a weight hung to the end of it: Upon the axis, to which the lever is fixt, is a pulley, ¼ of an inch in diameter, to which a piece of watch chain is faſten'd; the other end of this chain is hook'd to a ſtrong ſpring, markt G, which ſpring bears againſt one end of the metal E. H repreſents a lever exactly of the ſame form and dimenſions with the former; but the chain faſten'd to the pulley on

on its axis is hook'd to the ſtandard-bar. *N. B.* The chain to the former pulley being faſten'd to a ſpring, and not directly to the metal E, is only for the more eaſy ſhifting the metals. The line faſten'd to the end of this lever, after being wound round a cylinder, to which the moveable plate is fixt, paſſes over a ſmall pulley, and has a weight hung to the end of it; or rather the ſame line paſſing under a pulley, to which the weight is hung, has its other end faſten'd to the lever F: Thus one weight ſerves for both levers, as in the Fig.

From this deſcription it is plain, that whenever the bar E is lengthen'd, it gives liberty to the weight to draw the lever F upwards by its action on the ſpring G; and the index will at the ſame time, by means of the ſilk line, be carried forwards in the circle; and as the bar ſhortens, it will return back again; the ſame motion will be communicated to the ſtandard-bar.

Lengthening the bar the $\frac{1}{10}$ of an inch will carry the index once round the braſs circle, which is divided into 360 degrees; if, therefore, the metal lengthen the 7200th part of an inch, the index will move 1 degree.

To make an experiment with this inſtrument, lay a bar of any kind of metal, as E, on the ſtandard-bar; then heat the ſtandard bar to any degree of heat with a lamp, and mark the degree of its expanſion as markt by the moveable plate: Obſerve alſo the degree of expanſion of the metal E, by the heat communicated to it from the ſtandard bar, as markt on the braſs circle by the index: Let the inſtrument ſtand till the whole is thoroughly cold; then removing the bar E, lay a bar of any other metal in its place, and heat the ſtandard-bar to the ſame degree of heat as before, which is ſeen by the moveable plates marking the ſame degree of expanſion: Then the index will ſhew the degree of expanſion of the ſecond metal, as it did of the firſt; and by this means the degrees of expanſion of different metals by the ſame degree of heat may be exactly eſtimated.

A farther Account of the Bones *of* Animals *being made red by aliment only; by Mr.* John Belchier. Phil. Tranſ. N° 443. p. 299.

IN the account, *Phil. Tranſ.* N° 442. p. 286, which Mr. *Belchier* gave of the red bones of the hogs, he mentioned, that the colour was occaſion'd by mixing with their common food, bran made uſe of to clean printed callicoes; the colours of which were made, ſome from preparations of iron, as the blacks

blacks and purples; others, from preparations of alum and *Sachar. Saturni*, which produce the red colours; and that madder root was made use of to fix these colours on the cloth. To which of these preparations the colours was owing, he could not then determine. Some were of opinion, that it was entirely owing to the preparations of iron; others, that it was the whole blended together: And in order to clear up this point, Mr. *Belchier* determined to make some experiments. The first he made was upon a cock, by mixing some of the madder-root with fig-dust, on which they feed. — The cock dying within 16 days after his first feeding on the madder, he dissected him, and examining the bones, to his great surprise, he found them all over of a red colour: So that from this experiment it appears, that the madder alone causes this alteration.

Of the Viper-catchers, *and their remedy for the Bite of a* Viper; *by Dr.* Burton. Phil. Trans. N° 443. p. 312.

WILLIAM Oliver and his wife call'd upon Dr. *Burton* with their vipers, and either of them offer'd to be bit by any viper, and to suffer their arm to swell for some time; and then by the external application of a common cheap remedy, in a few hours to remove all the symptoms. Accordingly the experiment was made in the town-hall at *Windsor*, before Dr. *Derham*, Dr. *Waterland*, and before the physicians, apothecaries and surgeons of the town and several other Gentlemen in the neighbourhood. The man was bit in the upper joint of the thumb, and higher up on the same arm, by 2 different fresh vipers: His thumb, hand and arm soon after swell'd much, and all the usual symptoms of a viper bite ensued; he applied the remedy (sallad oil) with the promis'd success.

An Account of Experiments made June 1, 1734. *before several Members of the* Royal Society *and others, on a* Man, *who suffer'd himself to be bit by a* Viper, *or common* Adder; *and on other Animals likewise bitten by the same and other* Vipers: *With some Remarks on the Cure of the Bite of a mad* Dog; *by Dr.* Mortimer. Phil. Trans. N° 443. p. 313.

WILLIAM Oliver and his wife, from *Bath*, who follow the business of catching and selling vipers, offer'd themselves to be bit by any viper that should be procur'd, trusting to the virtue of a remedy they had lighted on by chance in trying variety of things, when the woman was once accidentally bitten, and the usual known medicines, even the oil of vipers, had

had no effect in aſſwaging her pain, eſpecially that of her breaſt on the ſame ſide with the hand in which ſhe had receiv'd the wound. This remedy which is only common *oil of olives*, and from its uſe with ſallad, commonly known by the name of *ſallad-oil*, recommends itſelf not only from its efficacy; but likewiſe on account of its being readily come at, when accidents happen.

On the 1ſt of *June*, 1734, in the preſence of a great number of perſons, the ſaid *William Oliver* was bit by an old black viper, or adder, brought by one of the company, upon the wriſt and joint of the thumb of the right hand; ſo that drops of blood came from the wounds. He ſaid that he immediately felt a violent pain and ſhooting from the wounds, both to the top of his thumb and up his arm, even before the viper was looſen'd from his hand: Soon after he felt a pain, like that of burning, trickle up his arm; in a few minutes his eyes began to look red and fiery, and to water much: In leſs than ½ an hour he perceiv'd the venom ſeize his heart with a pricking pain, which was attended with faintneſs and ſhortneſs of breath; whereupon he fell into violent cold ſweats: In a few minutes after this, his belly began to ſwell, with great gripings and pains in his back, which were attended with violent vomitings and purgings. He affirmed, that during the violence of theſe ſymptoms, his ſight was gone twice for ſeveral minutes at a time; but that he could hear all the while. He ſaid, that in his former experiment he had never deferred making uſe of his remedy longer than when he perceived the effects of the venom reaching his heart; but this time, being willing to ſatisfy the company thoroughly, and truſting to the ſpeedy effects of the oil, which had never fail'd him, when uſed in time, he forbore to apply any thing, till he found himſelf exceeding ill, and quite giddy.

About an hour and a quarter after he had been firſt bit, a chaffing diſh of glowing charcoal was brought in, and his arm (the cloaths being ſtript off of it) was held over it as near, as he could bear it, while his wife rubb'd in with her hand the ſallad-oil (which the Dr. had bought by the name of *Lucca* oil, and kept in his pocket, leſt they ſhould privately add any thing to it) turning his arm continually round, as if ſhe would have roaſted it over the coals: He ſaid that the pain ſoon abated, but the ſwelling did not diminiſh much; moſt violent vomitings and purgings ſoon enſued, and his pulſe became ſo low, and ſo often interrupted, that it was thought proper by the phy-

phyſicians preſent to give him the following cordial draughts at about ¼ of an hour's interval between each.

1. ℞ *Aq. Lact. Pæon. comp. aa* ℥ iij *Sp. Lavendulæ* ℥1. *m. pro duobus hauſtibus.*

2. ℞ *Confect. Raleigh.* ℥ ſs. *Aq. Theriacal.* ℥ ſs. *Sp. C. C. gut.* x. *m. f. hauſtus.*

3. R. *Confect. Raleigh. Theriac. Andromach. aa* ℥ſs. *Sal. C. C. gr.* v. *Aq. Theriacal.* ℥ij. *pro duobus hauſtibus.*

He ſaid he was not ſenſible of any great relief from theſe cordials, but that a glaſs or two of oil olive, drank down, ſeemed to give him ſome eaſe.

Continuing in this dangerous condition, he was put to bed, as ſoon as one could be got ready for him, where his arm was again bathed with his remedy over a pan of charcoal ſet by the bed ſide: But continuing to complain much of his back and belly, the Dr. advis'd his wife to rub them likewiſe with ſallad-oil, heated in a ladle over the charcoal; which ſhe did accordingly: Whereupon he declared he found immediate eaſe, as tho' by ſome charm; and he had not above two or three reachings to vomit, and ſtools afterwards, but made water plentifully, which was not diſcolour'd: Then he ſoon fell into a ſound ſleep, only was often interrupted by perſons coming to ſee and enquire after him till near 12 o'clock, from which time he ſlept continually to five or ſix next morning, when he awaked and found himſelf very well: But in the afternoon, upon drinking ſome rum and ſtrong beer; ſo as to be almoſt fuddled, the ſwelling returned with much pain and cold ſweats, which abated ſoon, upon bathing the arm as before, and upon wrapping it up in brown paper, ſoaked with oil.

Immediately after the man, two pigeons were bit by the ſame viper, they ſoon ſickened and ſeem'd giddy: Nothing being applied, the one died in about an hour's time, and the other half an hour after: The fleſh of both was turned quite black, as if mortified; the blood was coagulated and look'd black.

On the 3d of *June*, the man's arm remain'd ſwell'd, look'd red, marbled with ſpots of yellow, but felt ſoft; and he had the perfect uſe of it, and even of his fingers, no pain or ſtiffneſs remaining. He then cauſed a ſmall ſpaniel dog to be bit on the noſe by a freſh viper: Some oil was immediately applied hot, and rubbed well in, till all the hair of his noſe was thoroughly wet: The dog did not ſeem very

uneaſy

uneasy; his nose only swell'd a little; he eat soon after; his nose was bathed once more that evening; he was found very well next morning; but his nose was bathed again, to make sure of the cure: He continued perfectly well without any symptoms ensuing, and was alive and well a year after.

Another pidgeon was likewise bit under the wing at the same time with the dog, but by a fresh viper; the oil was immediately applied hot, and rubbed well in, and the feathers of the wing were thoroughly wetted with it: The pidgeon did not seem at all disorder'd with the venom, but eat soon after, and was found well the next morning, without any remarkable inflammation or swelling about the part. The hot oil was rubbed in again for two or three days, twice a day, and the bird continued well, which shews the efficacy of this remedy, since so small an animal receives the same quantity of venom by the bite as a larger one, and consequently is more liable to die under it: The viper-catchers said, that they had experienced their remedy to take effect on cows, horses, and dogs, 10 hours after being bit: But that for themselvess who are frequently bit in the fields, as they catch the vipers, they always carry a phial of sallad-oil along with them, that as soon as they perceive themselves wounded, they without loss of time bathe the part with it; and if it be the heel, they wet the stocking thoroughly with it; if the finger, which happens oftenest, they pour some of it into that finger of their glove, which they immediately put on again, and thus never feel any farther inconvenience from the accident, not even so much as from the sting of a common bee. It may, perhaps, be found of use for the bite of rattle snakes, and other venomous creatures; especially if we consider that in the fields a man seldom or never receives more than one bite at a time, which doth not infect him with so much venom, as was instill'd into the man's blood, when in these voluntary experiments he suffer'd himself to be bitten twice together, and had likewise been bitten three times, but about a week or 10 days before; some remains of which venom, it is highly reasonable to imagine, might still infect his blood at the time he repeated the experiments; so as to make a fresh quantity of the venom operate with greater violence upon his body, than if he had been quite a fresh man, and never infected with the like poison before; or at least at so great an interval of time, that

his blood might have been intirely free from all remains of such an acrid infection. From these experiments is it not reasonable to imagine, that the oil by itself may be as efficacious against the sting of a scorpion, as if scorpions were infused in it?

And now a remedy against the bite of the viper is as publickly known as the famous *Dampier*'s powder against the bite of a mad dog, which was first publish'd by Sir *Hans Sloane* in Phil. Trans. N° 237, and afterwards by his proposall to the *Royal College* of physicians in *London* in 1720, introduced into the London *Pharmacopœia*, under the name of *pulvis antilyssus*; the composition of which is *lichen cinereus terrestris*, or ash-colour'd ground liver-wort and black pepper; the manner of giving it not only to men, but to dogs and cattle, is accurately set down in the above-mentioned *Transaction*: So that now it is to be hoped, that certain cures are discover'd for the only two sorts of venomous bites of animals, to which the happy soil of *Great-Britain* exposes its inhabitants.

The *lichen cinereus terrestris* in a letter of Mr. *Oldenburgh*'s, secretary of the *Royal Society*, dated *July* 6. 1672. is mentioned as being exceedingly efficacious in curing dogs bitten by mad dogs. Vide *Derham*'s Collection of Philosophical letters between Mr. *Ray*, and his correspondents, p. 110, printed at *London* in 1718, 8^vo.

Dr. *Mortimer* adds the following passage taken out of the journal-book of the *Royal Society*, supposing it to be what Mr. *Oldenburgh* hints at in his letter.

'*Nov.* 16. 1671. Sir *Robert Moray* exhibited a certain 'plant, (which Mr. *Wray* calls *lichen terrestris cinereus*) said 'by him to be good to cure dogs bitten by mad dogs: His 'Royal Highness having caus'd it to be given to a whole 'kennel of dogs bitten by a mad one, which were all cured 'except one of them, to whom none of it was given.'

The specimen was kept in the repository.

The same virtue is likewise ascribed to this plant in the 3d part of *Morison*'s *Plantar. Histor. Oxon.* published at *Oxford* Ann. 1699, in folio, p. 632, where the author speaking of the *lichen terrestris cinereus*, *Raii Hist. & Synops.* says, *adversus morsum canis rabidi egregium est medicamentum.*

Dampier and the *College of Physicians*, in their *pulvis Antilyssus*, prescribe equal quantities of the *lichen* and pepper: But Dr. *Mead*, in a single quarto leaf published by him, in

1735

1735, hath alter'd the proportions of the compoſition, preſcribing double the quantity of *lichen* to that of the pepper. This difference in the proportions muſt be left to the judgment of practitioners: But upon the authority of another minute in the Society's Journal-book, it may not be improper to make an addition to the above mention'd *Dampier*'s powder.

'*March* 7. 1671-2, Sir *Robert Moray* mentioned, that a 'whole kennel of dogs, belonging to his Royal Highneſs, 'were bitten by a mad dog, and had been lately cured by a 'certain herb, call'd *ſtellaria*, or *ſtar of the earth*.'

This plant is the *lychnis viſcoſa, flore muſcoſo Caſp. Bauhin*, or *Spaniſh catch-fly*. Vide *Phil. Tranſ.* N° 187, where is a receipt to cure mad dogs, *&c.* wherein this plant is a principal ingredient; which receipt, communicated by Sir *Robert Gourdon*, was there publiſhed by his Majeſty's ſpecial Command, *Ann.* 1687.

Wherefore ſuppoſe the compoſition were to be thus.

Take aſh-colour'd ground liver-wort, black pepper, and the herb *Spaniſh catch-fly*, all finely powder'd, of each two drachms for four doſes, to be taken as *Dampier* preſcribes in *Phil. Tranſ.* N° 237.

Dr. *Mortimer* only adds a propoſal of his own, which he made in his *Theſ. Inaugur. de ingreſſu humorum in corpus humanum*, Lugd. Bat. 1724. namely, that the uſe of the hot bath for perſons bit by a mad dog, or hot fomentations, might be of greater ſervice than cold applications: For, a cold bath ſhuts the pores, as a warm one opens them; therefore, the blood being allowed to be greatly inflam'd in this caſe; and *Dampier*'s powder being a very hot medicine, it is reaſonable to think, that when a patient takes it, the ſetting him up to the chin in hot water for ſome hours, would help the operation of the powders, by diluting the blood, and relaxing the pores.

Of an inguinal Rupture, with a Pin in the Appendix Cœci *incruſtated with ſtone; and ſome Obſervations on Wounds in the Guts; by Mr.* Claudius Amyand. Phil. Tranſ. N° 443. p. 329.

Octob. 8. 1735. A Boy, 11 years of age, was admitted into St. *George's Hoſpital*, near *Hyde-Park-corner*, for the cure of a *hernia ſcrotalis*, which he had from his infancy, and a *fiſtula* between the *ſcrotum* and thigh

terminating into it, which, for a month before, had discharg'd a great quantity of an unkindly matter: The rupture was ſmall, and not troubleſome, and part of it could be replaced; but as it appear'd that the ſinuous ulcer ſprung from that part that could not; ſo it was evident that the cure of the *fiſtula* depended upon the cure of the *hernia*, which latter could be obtain'd by no other operation than that for the *bubonocele*, which was accordingly performed the 6th of *December* following.

This operation prov'd the moſt complicated and perplexing Mr. *Amyand* ever met with, ſeveral unlook'd for oddities and events concurring to make it as intricate, as it prov'd laborious and difficult.

This tumour, principally compos'd of the *omentum*, was about the bigneſs of a ſmall pippin: In it was found the appendix *cœci*, perforarated by a pin, incruſtated with ſtone towards the head, the point of which having perforated that gut, gave way to a diſcharge of fæces through the fiſtulous opening therein, as the portion of the pin, obturating the aperture in it, ſhifted its ſituation. The abſceſs, formed in the hernial bag occaſionally, and the ſuppuration for the two months before from this place externally, had knit and confounded, and embodied together, as it were, the gut and *omentum* with the hernial bag, and theſe with the ſpermatic veſſels, and the teſticle; ſo that it was as difficult to diſtinguiſh them from each other, as it was to ſeparate them without wounding them; this pin, whoſe point was fix'd in the *omentum*, continually ſhifting its ſituation, and occaſioning a diſcharge of *fæces*. The pin frequently lying in the way of the knife, and ſtarting out of the wounded gut, as a ſhot out of a gun, the inundation of fæces upon this occaſion, from a gut we could not well diſtinguiſh, were ſo many difficulties in the way. But the greateſt ſtill was, what to do with the gut, which all this while was unknown, and which we could not come to the knowledge of, till the operation was over: For, this *appendix cœci*, which was the only gut found in the rupture, was ſo contracted, carnous, duplicated, and changed in its figure and ſubſtance, that it was impoſſible to determine what kind of gut it was; or to find out that it was only this *appendix* elongated, and in diſguiſe.

None

None of these difficulties were apprehended, when we undertook the operation, in which we proceeded as usual: The *omentum* lying uppermost in the hernial bag was separated from the parts it was connected with, and particularly the gut it was embodied with, and afterwards cut off close to the abdominal muscles, without any previous ligature, the vessels in it being small, and the substance of it more like a sweat-bread than a caul.

A deal of time was spent in this dissection: We were straightened for room, and greatly disturbed by the discharge of the fæces coming out of the gut, upon every motion the pin, lodged therein, and the *omentum*, suffered, upon the separation of these from each other. The gut forming a double tube, like a double-jointed siphon, continuing in the curve, as it passed over the testicle and spermatics, was separated one part from the other and from the adjacent parts, as far as the aperture in the abdominal muscles, where the unperforated part of it was separated therefrom, and thence stretched out and unfolded; which brought to view the aperture made in it by the pin, hitherto concealed, thro' which that part of it, which was incrustated, had just made its way out upon an occasional pressure, as a cork out of a bottle. It was the opinion of the physicians and surgeons present, to amputate this gut: To which end a circular ligature was made about the sound part of it, two inches above the aperture; and this being cut off an inch below the ligature, was replaced in the abdomen, in such a manner that an artificial anus might be made there, if the patient's case should require it. Afterwards so much of the hernial bag as had been detached from the skin, and spermatics, *&c.* was cut off, which, as they appeared in a sound state, were preserved in their site. The fistulous opening adjoining to the thigh, and answering to the aperture in the gut, was opened; some angles of skin in the way were removed; the aperture in the muscles, which had been enlarged by incision, was stopped up with a tent; and the rest of the dressings, and the situation of the patient, ordered in such a manner, as to remove from the wound all such pressure from within, as might disturb the cure.

This was a continued dissection, attended with danger to parts not well distinguished; it lasted near half an hour, and the patient bore it very courageously. During the operation the patient vomited plentifully, and had several stools, but was soon composed by half an ounce of diacodium, and emollient embrocations and fomentations, frequently applied warm on the belly:

belly: He was blooded, and an emollient carminative oily clyſter was ordered to be applied in the evening; but as he was eaſy, and the belly not tenſe, that was omitted. He was confined to a very ſpare diet, and his body kept open by clyſters, injected every ſecond day, when ſtools were wanted, to prevent ſtraining. When dreſſed upon the fourth day after the operation, every thing appeared well, and we had good reaſon to hope for a cure, eſpecially as the diſcharge by the anus was natural. The tent put into the abdominal aperture was not removed till the eighth. On the tenth the ligature round the *appendix cæci*, where it had been amputated, dropped off, and no fæces followed it; and as it was then plain they had taken the natural courſe, from that time the wound was treated like an ordinary one, only it was obſerved to keep a ſtrong and conſtant preſſure over the abdominal aperture, as well to fence againſt the intruſion of the *viſcera* into the wound, as by a ſtrong incarnation and cicatrice, to ſecure the patient effectually againſt a rupture. During the time of the cure he was confined to his bed, always kept to ſpare diet, and ordered never to go to ſtool but in a bed-pan: By theſe means the wound was compleatly healed up in leſs than a month, and the patient ſoon after diſcharged out of the *Hoſpital* with a truſs, which he was ordered to wear ſome time, in order to confirm the cure.

That the *appendix cæci* ſhould be the only gut found in this rupture, is a ſingular caſe in practice; this was full of *fæces*, and could occaſionally be diſtended with an additional quantity, which upon preſſure was returned into the *colon*, with that kind of noiſe which guts replaced generally give: This had occaſioned a diminution of the tumour when compreſſed, before the operation was performed, as the patient was lying backwards with his head downwards, and an increaſe of it as he ſtood erect, when the *fæces* from the *colon* could get into it again.

The patient did not remember when he ſwallowed the pin which had perforated the gut within the rupture: But as this rupture was, from his infancy, fixed and unreducible; ſo it is likely the pin had then made its way into the *appendix cæci* prolapſed; and that an inflammation enſuing thereon, had occaſioned an adheſion, whereby the increaſe of the tumour had been checked, and the reduction of the parts, prolapſed thereby, rendered impracticable.

The ſurgeons who conſtantly dreſſed the patient before the operation, did then obſerve, as they had ſince, that the humour, diſcharged formerly at the fiſtula, had frequently the appear-

ance,

ance, and as they thought, the ſmell of excrement: So that there is no doubt but the cauſe of it was the wound made in the gut, by the pin giving way occaſionally to ſuch a diſcharge. The patient alſo perfectly remembers, that the impoſtumation or gathering preceding the fiſtulous diſcharge was attended with very little pain, or much leſs than generally attends ſuppuration; which ſhews that the extravaſation of the *fæces* from the gut into the hernial bag, and the burſting of this bag, were the cauſe of the fiſtulous diſcharge, and the continuance of it outwardly.

As to the pin found in the rupture at the time of the operation, it is obſervable, that two thirds of it, incruſtated with a chalky matter, were confined and concealed within the gut; the other third next the point, had made its way through it; the point of which was ſo lodged in the *omentum* wherein it was fixed, as to leave a free paſſage for the excrement from the perforated gut outwardly, whenever the perforation in the gut, upon ſhifting the poſition of the incloſed pin, could open, and give a paſſage for the diſcharge of the *fæces* this way, which was as often as this conical or pyramidal pin altered its place, or did not exactly obturate the aperture in the *appendix cœci* which it exactly fitted. It has been already obſerved, that the aperture, made in the gut by the pin, lay concealed, the point being lodged in the *omentum*, lying parallel with the gut, which was here duplicated, where it was ſecured in ſuch manner, that it ſeemed almoſt impoſſible it could ever make its way out of this place, and its other confinement in the gut, as the aperture was callous, and ſo reſiſting, that it was with ſome violence it was forced out of its confinement through an aperture, fitted for the point only, and ſo ſtreight, that the exploſion upon its coming out was like that of a cork out of a bottle: For, tho' it appeared that the opening had been occaſionally enlarged, as the incruſtated part of the pin was preſſed forward into it; yet it is plain nature's attempts to get rid of it had been fruitleſs; and might poſſibly have been ſo during all the patient's life.

Sir *Hans Sloane* has furniſhed the curious with inſtances of bodies incruſtated in the guts with ſtone, and of ſome making their way out, when there was little probability of it. Daily experience ſhews how far nature will ſtruggle to free herſelf; ſo that it is always moſt eligible to truſt them to her care: This may appear from the difficulties that have attended the cure of this caſe, which at laſt did not prove ſo ſucceſsful as was at firſt hoped for: For, the patient having been remiſs in wearing his truſs,

truſs, upon ſome effort the guts found a way into the groin again, ſix months after the healing of the wound. This caſe likewiſe ſhews, that the beſt operation, and the utmoſt care, are no ſecurity againſt the relapſe of a rupture. This is the third or fourth inſtance Mr. *Amyand* has met with, of the inſufficiency of this operation to effectuate a cure of ruptures; and yet it is plain this is by far more likely to prove effectual, than the cauſtic or any other method cried up for the cure of this evil. In a growing age, a good ſpring truſs is an effectual remedy; and in an adult, this ſhould be the ultimate one, tho' it be no more than a palliative cure.

N. B. The *omentum*, and the gut, amputated, with the pin perforating it, are in the repoſitory of the *Royal Society*.

This obſervation puts Mr. *Amyand* in mind of two he made during the late war in *Flanders*, and of two more in *London*.

Obſervation 1. Upon opening the body of a ſoldier, who had laboured many years with an inguinal and ſcrotal rupture; Mr. *Amyand* found in a ſegment of the *ileum*, an appendix like a *cœcum*, about ſix inches long, ariſing from that gut, and nearly of the ſame diameter with the gut itſelf, the coats whereof were ſomewhat thinner than thoſe of the *ileum* this *cœcum* did ariſe from, whoſe membranes and dimenſions were natural. This elongation of a ſegment of the *ileum* appeared, as if it had been lodged in the rupture bag it lay near to, and into which it had been ſtretched along the *vagina* of the ſpermatic veſſels down to the teſticle, according to the expanſion of the rupture bag, which was of the ſame dimenſion: This production of the *ileum*, or appendix *ilei*, was full of *fæces*, ſomewhat narrower at its origin or opening into the *ileum* than elſewhere, but nearly reſembling it, and as ſound as that gut it ſprung from.

Obſ. 2. A ſoldier having been ſhot through the belly, the ball was cut out upon the poſterior part of the *os ilium*: For ſeveral months after the *fæces* were chiefly diſcharged through both wounds; and at dreſſings a great number of flat worms, dead or alive, were found upon the plaiſter. In five or ſix months after, the *fæces* having by degrees taken their courſe thro' the anus, the two wounds being healed up, the patient returned to his duty as a ſoldier.

Eight years after this, Mr. *Amyand* had him again under his care at the hoſpital, where he was brought with the head of the *os humeri*, together with that of the acromion and clavicle, in the articulation with the *ſcapula*, fractured by a cannon ſhot; which thereby was all laid open: The limb was immediately

cut

ut off in the articulation with the *ſcapula*, having firſt made a ligature about the fleſh ſurrounding the veſſels, by thruſting loſe to the bone a pack-needle, armed with a ſtrong pack-hread, there being no room for the *tourniquet* (vide M. *Le Dran's Chirurgical Obſervations*, Vol. II. Obſ. 43. 12°. 1731. where he has deſcribed the manner of performing the amputation of the *humerus* in the articulation with the *ſcapula*) The patient loſt very little more blood in the operation, than if a *tourniquet* had been applied: But the great diſcharge of matter ank him, and he died the eighth day after.

The death of this patient gave Mr. *Amyand* an opportunity f examining how the former wound in the gut had been ured. He thought the wound had been in the *ileum*, from he thinneſs of the *fæces* diſcharged through that wound; but pon diſſection he found it had been in the broadeſt part of the *olon*. This was very much contracted in that part of it that ad been ſhot through, where it appeared purſed up, and inſeparably connected with the *os ilium*. However, the patient ner complained of any inconveniency therefrom, tho' the narowneſs of the gut in this place was ſuch, as ſeemed to make the eſcent of the *fæces* difficult.

Obſ. 3. On the 19th of *January*, 1729, Mr. *Amyand* attended Miſs ——, 14 years of age, on account of a ſuppurated umour on the navel, whoſe ſituation was under the *muſculi recti*. This patient had had, what is truly called, a ſtarting at the avel in her infancy; and latterly had complained, at times, of ſwelling there, and likewiſe of colics, gripes, or vomitings, hat uſed to go off, particularly as that ſwelling diſappeared. As theſe grew more troubleſome, ſhe latterly had taken a vomit, rom which time ſhe had been greatly coſtive; and her reachngs, vomitings, and colics, had proved more conſtant, together vith an increaſing tenſion and pain in the forepart of the belly, nd a tenderneſs at the navel, as matter was gathering there.

Some days before Mr. *Amyand* was called in, Dr. *Campbell* ad made uſe of the propereſt methods to remove theſe complaints. Upon a conſultation, we agreed to diſcharge by inciion the matter collected at the navel, being about a ſpoonful of ndigeſted fluid, that had made its way through the *aponeuroſis* f the abdominal muſcles adjoining to the navel *cicatrice*: Notwithſtanding which, the tenſion of the belly, the coſtiveneſs, the eachings and vomitings, rather increaſing, as in the *miſerere ei*; and having thence reaſon to apprehend a ſtrangulation nd ſuppuration of ſome of the *viſcera* in the neighbourhood of

the navel, Dr. *Hollings* being call'd in, it was agreed to inlarge the aperture made by the aforementioned matter in the *linea alba*, with a view and intent to know the ſtate the parts were in, to reduce what we found there, or at leaſt to procure a more free diſcharge to the matter collected under the *aponeuroſis* of the muſcles: For a fortnight and more, every thing was done that could internally or externally eaſe the diſcharge, and open the paſſage for the *fæces* downwards, but all in vain. The patient was a whole fortnight without a ſtool, all the ſymptoms daily increaſing, tho' towards the latter end ſhe vomited rather more ſeldom: Yet as ſhe was ſtill taking in, ſo the dimenſions of her belly increas'd in proportion; and the more for that the air confined and rarefied in the *fæces* pent in, added daily to the tenſion; which at laſt had ſtretch'd the ſkin to the utmoſt. There was alſo a ſuppreſſion of urine, the *fundus* of the bladder being ſtretch'd towards the navel, at the ſame time that the neck of it was compreſſed by the *fæces* bearing down in the *pelvis*, and a tumour ſprung up about the *anus*, as if they had been ſeeking a paſſage that way. It was propos'd to ſcoop them out; but the *rectum* was found empty, and the obſtruction was as far beyond the reach of any chirurgical operation, as it had prov'd againſt all the means hitherto employ'd.

The patient was now reduced to the loweſt ebb. The dejections were excrementitious, her pulſe depreſs'd and extremely weak; ſhe had rigors, clammy ſweats, and all the ſymptoms that denote an approaching death, from a mortification in the guts, when of a ſudden the *fæces* burſted the gut, and forcing their way thro' the inciſion at the navel, a quantity equal to 2 or 3 quarts, mixed with various kinds of fruits and ſeeds which ſhe had been taking during her illneſs, flow'd out like a torrent, with a ſurpriſing roaring noiſe, which gave her immediate relief. The diſcharge continued very great all that day, but the aperture in the hernial bag did not anſwer to that in the gut; ſo that the diſcharge there was at times checked by ſubſtances obturating it; this aperture, therefore, was enlarged by inciſion; and thereby the patient was releas'd from the violence of the vomiting and hickup. From this time we began to entertain ſome hopes of a cure: For, tho' the patient was extremely reduced, and the diſcharge continued exceeding great during ſeveral days, with a *ſingultus* and vomitings; yet ſhe was refreſh'd with ſleep, and was able to retain ſome nouriſhment. The tenſion of the belly ſubſiſted, tho' in a leſſer degree till the *fæces* had made their way downwards; and ſo did the vomi

vomitings at times, as long as the inflammation continued. The diet was such as the case required; clysters were frequently applied, as well as fomentations, and every thing else that could determine or invite the discharge thro' the *anus*, and restore the distended guts to their tone: But from the time the *fæces* bursted the gut, it was 12 days before any took the natural course; and then matters were brought to the brink of ruin: For, they then pour'd down so fast for a day or two, that the patient was like to have sunk under them: However, this severe evacuation was timely conquer'd by absorbents and diluents: It took off the remaining tension of the belly and all vomitings: And as from this time the *fæces* had a free discharge the natural way, and the discharge thro' the wound decreas'd in proportion; so the wound in the gut, and the external wound in the integuments, were heal'd up in about 3 weeks, in such manner that the patient has ever since enjoy'd a most perfect state of health.

Obs. 4. It happen'd that Mr. *Amyand* was not a mere stranger to the principal circumstances of this case, as in 1716 he had with Mr. *Lafage*, surgeon, attended such another, *viz.* a girl about 4 years of age, in whom the same cause had produced the like effects, For, upon a suppuration of the *omentum*, strangulated in the navel of this patient, the *fæces* detain'd in the neighbouring gut had in like manner forced their way thro' the navel: The accident previous to the bursting and subsequent upon it, having been nearly the same as in the preceeding observation; only the cure prov'd somewhat more tedious: For, the wound was kept open by curran-seeds frequently working their way out at the navel for about 12 months after, when it was made complete: So that the hardships the patient has undergone since in child-bearing, and several hazardous labours, have not been able to disturb it.

Hence it appears, that the parts inflamed and in contact have coalesced and knit together; so as to prevent any extravasation from the wounded or burst gut into the cavity of the *abdomen*.

That the cure in the 2 last cases has been owing to a free discharge of the *fæces* thro' the wound; and consequently, that when in a gut-rupture, the part prolapsed cannot be reduced, a cure may be hoped for by making such an opening in the guts, before they are quite sphacelated, as may procure a free discharge to the *fæces* pent in, and thereby secure the patient's life.

That if this happen to the *colon* or *cæcum*, the tube of it will be so far preserv'd, as to open a free discharge for the *fæces* the natural way; and if that cannot be obtain'd in a wound of the

ſmall gut; yet the diſcharge may be ſecur'd by making the wound an artificial *anus*.

That the readieſt way to obtain a cure of a wounded or burſted gut, is to keep it in contact with the external wound and the patient at a very low diet.

That the deligation of the veſſels of the *omentum*, previous to amputation, being liable to ſeveral exceptions, it is more eligible to forbear it, ſaving when the veſſels are large: For, when reduced looſe and floating, it is leſs liable to the inflammations and ſuppurations that attend the ſeparation of the ligature.

A Continuation of the Experiments on Mercury; *by Dr.* Boerhaave. Phil. Tranſ. N° 443. p. 343. *Tranſlated from the* Latin.

BY the obſervations made on quickſilver *Phil. Tranſ.* N° 430. p. 145. it appears, that tho' it ſeem continually to change into other bodies, it ſurpriſingly retains its peculiar property, whereby it is immutable.

Dr. *Boerhaave*'s ſole deſign at that time was to give a faithful and accurate account of the proceſſes he had made on mercury and the effects produced therefrom; in order to ſave others the trouble and expence of repeating them.

In the *Memoirs of the Royal Academy of Sciences at Paris* for 1734, the Dr. publiſhes farther experiments on the ſame ſubject: And by comparing both theſe diſſertations together we ſhall be enabled to form a judgment of the fidelity and minute accuracy of the ancient and genuine alchemiſts, as to what they may have ſaid of mercury; and at the ſame time ſee, that great care and prudence are neceſſary to explain their meaning, or paſs a juſt critic upon them: And he alſo hopes to put the ſtudious of chemiſtry upon their guard againſt the vague doctrine of modern alchemiſts, which contains nothing valuable or ſolid, and who themſelves are maſters of no other art than that whereby they would cheat thoſe of real gold, whom they would perſuade that they can inſtruct in the true method of making it: What the Dr. had formerly demonſtrated of the immutability, ſimplicity and peculiar properties of mercury, he now comes to confirm,

1. Pure mercury, ſuch as is commonly ſold by the company at *Amſterdam*, digeſted for ſome time over the fire, does not change into metal.

Proceſs. Upon diſtilling this mercury, it left no dreg behind; mixing it afterwards with diſtill'd vinegar and ſea ſalt, and

and ſhaking it for ſome time, it continu'd pure. Straining it thro' leather, and pouring a pound of it into a tall clean body, whoſe mouth was ſtopt with a paper cone, which was again covered with another paper, faſtened well to the neck of the body, that no duſt might enter, but the air paſs and repaſs freely; he committed it to a conſtant degree of heat, which by M. *Fahrenheit*'s thermometer was kept up from the 15th of *November*, 1718, till the 23d of *May*, 1734 above the 100th degree; and he found the mercury in a fluid ſtate, with a ſmall quantity of a black powder on its ſurface; and grinding this powder in a mortar, it revived into mercury. He put this whole quantity of mercury into a clean glaſs retort in order to diſtill it; and heightning the fire towards the end, till the retort became almoſt ignited, there remain'd nothing at all in the retort; for, the mercury came over without any ſenſible alteration.

Corollaries. 1. Fire, heighten'd to the degree and continu'd for the time above-mentioned, cauſes no alteration in the fluidity, volatility or nature of mercury, put into a veſſel, where the air has free acceſs to it: nor is there any ſeparation made of the pure from the impure.

2. Nor any ſenſible generation of metal in the ſmalleſt quantity.

3. Much leſs of gold or ſilver.

4. None of the mercury was by this proceſs fixt, tho' continued for 15 years and $\frac{1}{2}$; nor was there the leaſt appearance of a beginning metallic fixation, nay not of lead; which, according to thoſe who value themſelves upon knowing theſe things beſt, is the firſt metal that ſhould be formed from this proceſs.

5. This proceſs does in no manner favour the opinion of ſuch as affirm, that metals are formed from mercury, as the matter, and from fire, as the fixing ſulphur united together by digeſtion.

6. It ſeems entirely probable that all ſuch proceſſes, made with common genuine quickſilver, do nowiſe tally with what authors promiſe about them; ſince that ſmall quantity of black powder, above-mention'd, is lighter than the mercury on whoſe ſurface it floats, and very eaſily becomes mercury again: The reader may ſee in *Phil. Tranſ.* N° 430 what has been ſaid on the like kind of black powder, produced by ſhaking only.

7. It

7. It does not appear, that mercury in the mines can be changed into any metallic matter by the ſole action of the ſubterraneous heat acting for ſome time, and in a place where the air has free acceſs; for, the heat, in places where metallic veins are found, ſeldom exceeds the 70th degree. They ſay, indeed, that 1000 years are requiſite to produce this effect; but how could mortals, who are ſo ſhort liv'd, have known this with certainty.

8. As to ſulphur, which the alchemiſts have taken for one of the principles of metals, and of which they ſay, that it unites the elements of mercury, that it may become a ſolid body and fixt in ſuch a degree of fire as may fuſe and render it malleable: This ſulphur ſeems to differ entirely from that matter of light or fire; tho' fire alone be the only inſtrument of producing this ſurpriſing union between ſulphur and mercury.

In this proceſs, however, the air had a free admiſſion to the mercury; and perhaps it might be alledged that this is the very thing that hinders the action of the fire; the rather as Alchemiſts aſſert, that crude air hinders the ſophical coction; and this induced the Doctor to make the following experiment.

9. Mercury put to digeſt in veſſels cloſely ſtopped for the time mentioned below, produces no metal.

Proceſs. The Doctor put pure mercury into a conical glaſs veſſel with a flat bottom, ſuch as Aſſayers make uſe of to ſeparate gold and ſilver, and expos'd it to a heat of 100 degrees from the 6th of *December* 1732, till the 8th of *June* 1733, and the veſſel, being always cloſely ſtopt, ſuffered no remarkable change; he took ſix ounces of this mercury, and put them into a veſſel like the former, into whoſe mouth he inſerted the neck of an inverted bolthead; without luting the veſſels, he expos'd the mercury for four days to a ſand heat, ſo intenſe, that the mercury began to aſcend, in order to evaporate all the moiſture it might contain: And when there ſeemed to remain not the leaſt ſign of humidity, he luted the junctures exactly. He expoſed the mercury to a pretty ſtrong ſand heat, whereby it aſcended and deſcended gently: He continued this degree of heat till the 29th of *January* 1734. In the bottom of the veſſel he found nothing but fluid mercury, ſtrewed lightly over with a light, ſubtile, black powder, without any thing fix'd or precipitated, tho' the degree of heat was nearer that of boiling water: He afterwards paſſed this mercury

fury through a clean, dry, paper funnel, whose lower orifice could scarce pass a hair.

The purest mercury passed thro' this small hole; and on the sides and about the hole of the funnel, there remained a small quantity of a black matter, which ground in a mortar, became mercury again. He committed the mercury thus depurated to a sand heat, in a clean glass retort for distillation, and at the last to a fire of suppression: There remained nothing fix'd in the bottom of the retort; the mercury appeared somewhat more fluid than before, but in other respects it was nowise changed.

Corol. Hence we may conclude the same things as from the preceeding experiment; and if to these we add what has been said *Phil. Trans.* N° 430, it will plainly appear that mercury in its own nature is invariable either by shaking it mechanically, or by distilling and digesting it as above described. From all which the Doctor concludes, that chymists may save themselves the needless trouble of repeating these processes, whereby they endeavour to fix mercury, or change it into some other body; and he advises them to be on their guard against ignorant pretenders.

The Doctor here gives an account of the success of other laborious experiments, he made on metals, besides the preceeding ones. He labour'd long to discover whether metals can by art be resolv'd into mercury, and into another principle; a great many authors affirm this so distinctly and in so many places of their writings, that the Doctor saw no cause of questioning the thing itself: He took it upon the credit of these authors; but to be convinced by his own eyes, he made trial on lead. The celebrated *Joh. Baptista Van Helmont. in potest. medicam.* §. 40, speaks to the following purpose; 'I found, *says he*, that the crudity of lead was soluble by 'the unctuousness of fixt salts, and sometimes deleble by fire 'only; and thus may the parts of the compound be divided, 'and the crude mercury permitted to run:' His son *Francis-cus Mercurius van Helmont* in his paradoxical discourses, *London* 1685, 8^vo. Part II. § 22. p. 111. speaks as follows: 'When lead is dissolv'd by alcalis and salts, or oils, which 'take in the sulphur, and separate it from the body, 'the lead by this means is changed into a volatile, running 'mercury, which can no longer endure the fire, as before, 'but is cold and running like water, and without a metalline 'form.' *Joachim Becher* affirms the same thing, and promises

miſes ſucceſs to a great many experiments he deſcribes to this purpoſe; vide *Collectanea quingentorum experiment.* p. 310—333. The following is a diſtinct and brief account of what Dr. *Boerhaave* learned about this matter, after a tedious and long continued proceſs.

Proceſs. As much pure ceruſs, as well could, was diſſolv'd in ſpirit of nitre, diluted with ſix times its weight of water; this ſolution he filtred, and it was found exceeding clear. From this liquor, put into a clean glaſs veſſel, and inſpiſſated by a gentle heat, and afterwards put to ſtand in a cool place, were form'd cryſtals, 14 ounces of which he took, and ground to a powder in a glaſs mortar, with a glaſs peſtle. This powder he diſſolv'd in the pureſt rain-water, and diluted the ſolution with thrice its quantity of rain-water; he afterwards pour'd on gently and carefully another filtred, and very clear ſolution of ſal ammoniac in rain water: This mixture becomes white like milk, and the lead immediately precipitates, as happens to ſilver diſſolv'd in *aqua fortis*, upon the admixture of ſal ammoniac. The powder precipitated to the bottom, which was as white as ſnow, elixated, and afterwards dried, became very inſipid, and weigh'd 18 ounces and ½. He put ſix ounces of this white and dry powder into a very clean glaſs urinal and pour'd on it, to the height of two inches, a very ſtrong lixivium of quick-lime and pot-aſh, which he had kept for ſeveral years in a phial cloſely ſtopped; he afterwards cover'd the urinal with filtring paper tied faſt about its neck, and put it into a heat of digeſtion of 96 degrees, where he left it from the 6th of *Frebruary* 1732, till the 15th of *Auguſt* following, to try whether this mixture, expoſed to the open air, would be changed by a heat of digeſtion; he found nothing but a white maſs, which reduced to a powder, taſted of ſalt; this he put into a glaſs retort coated over with a compoſition of loam and ſand, and urged it with a naked fire to ignition, and continued the ſame degree of heat for three hours: There aſcended into the neck of the retort a little white ſoot, but no mercury at all, and at the bottom remained a brittle ſemivitrified matter of a cineritious colour; this he reduced again into a powder of the ſame colour, and ground it for ſome time in a mortar with a lixivium of fix'd alcaline ſalt and quick-lime, and dried it again by a ſlow fire; he pour'd on freſh alcali, and expoſed it from the 18th of *Auguſt*, 1732, till the 15th of *October* 1733 to a heat of 96 degrees, grind-

grinding it daily in a glaſs mortar, which being cover'd over with paper, freely admitted the air: It was then a white, dry, and ſharp powder; which after pouring on the ſame lixivium a ſecond time, and grinding it, was reduced into a paſte; he committed it to a heat of putrefaction, as above, grinding it ſeveral times from the day above mentioned, till the 21ſt of *February*, 1734. Then it was a ſaline, white maſs, and nearer the taſte of ſea ſalt. After it had been ground well elixated, and very ſlowly dried, he found a very inſipid white powder, which he put into a retort, and kept for ſeveral days in the ſtrongeſt fire the luted glaſs could bear. On the 20th of *May* 1734, there came over no mercury; the neck of the retort was tinged with different colours; the friable maſs that remained at the bottom, exhibited likewiſe various colours diſpos'd in layers, and weigh'd five ounces ſix drachms and $\frac{1}{2}$: The powder, into which the maſs was reduced, look'd of the colour of reddiſh aſhes.

Scholium. In this proceſs, 1. The lead became ceruſs penetrated and diſſolv'd by the vapour of vinegar, and reduced to a white calx and afterwards to a fine powder. 2. It was diſſolv'd in diluted ſpirit of nitre; and thus the lead became a very clear, ſweet, colourleſs liquor, in which it was reduced and divided into its minuteſt particles. 3. The diſſolv'd ſal ammoniac poured on it by expelling the ſpirit of nitre, ſubſtituted in its ſtead ſpirit of ſea-ſalt, and intimately uniting with the metallic part of the lead, diſpos'd it as much as poſſible to expedite the ſeparation of the mercury from the metallic part, according to the opinion of thoſe who are ſuppoſed to have writ beſt on theſe matters: For, they principally aſcribe to ſal ammoniac and ſea-ſalt the property of ſeparating mercury from metals. 4. We have ſeen that the calx thus prepar'd, and kept in digeſtion for 7 months with the ſtrongeſt alcali ſhould have render'd the mercury manifeſt, by abſorbing the *ſulphur* of lead: Yet tho' a great degree of fire was applied, it did not yield the leaſt mercury. 5. This maſs being ſtrongly ground for ſome time, and afterwards mix'd with the ſtrongeſt freſh alcali, and digeſted for 14 months, exhibited no ſign of mercury. 6. It was again ground with freſh alcali, and digeſted for five months; ſo that after theſe proceſſes it was ſufficiently expoſed to the action of the alcaline ſalt, to have time to ſeparate the ſulphureous part of the lead, and to have the mercury, freed from its ſulphur by the force of fire, extracted

from this mass. However after all this labour, the strongest degree of fire extracted no mercury.

It therefore appears that what authors have boldly pronounced of the facility of extracting mercury from lead, is not confirmed by experience: For, say these authors, lead is a metal containing a great deal of mercury, which by the resuscitating salts very readily resolves into mercury. The thing is, therefore, more difficult in other metals: Yet authors promise themselves that this can be done pretty easily, and they prescribe methods differing little from that just mentioned, and from which after so much pains the Dr. learn'd, that the event was by no means so successful as they promis'd: And the Dr. very much questions, whether the assertions of these authors on this head be sufficiently supported by observations: And he is rather inclined to think that they have given into their own opinion on this matter, than consulted experience. What has been hitherto said will at least serve to save the reader the trouble and expence of repeating these observations, and prevent his easily admitting pretended principles of metallurgy. It were very much to be wish'd, that these ingenious and laborious chemists would give us a faithful account of the success of such experiments which frustrated their hopes, and never prescribe us processes, before they have tried them themselves; which would be a saving of time, expence and labour, and chemistry in a short time would obtain a place among the sciences: Otherwise we shall never arrive at truth, which is the sole aim of chemical disquisitions.

3. *Isaac Holland* writes, that mercury may be easily extracted from the salt of lead, made by distill'd vinegar. Dr. *Boerhaave*, having a mind to make trial of the best litharge and distill'd wine vinegar, prepared the concrete juice call'd *succus saturni*; two ounces of which he calcin'd in an open glass vessel by a gentle fire continued from the 6th of *June* 1734, till the 19th of *July* following: The white powder, got from it, was with a glass pestle ground very fine in a glass mortar. The trituration was very quick, and continued for some time, and at times pouring a lixivium, saturated with a very strong fixt alcaline salt, as much as the water could dissolve; he kept it in the same mortar covered over with paper at a continued heat from the 21st of *July*, till the 27th of *November*. As soon as this powder was dried, he took care to pour fresh lixivium to it, and grind it all

the

the time. By drying, wetting, and grinding it alternately all that time, he kept it cover'd over with paper at a heat of 90 degrees. On the laſt day of the proceſs he reduc'd this dry and white matter to an impalpable powder, and put this powder into a luted glaſs retort, and expoſed it for four hours to a fire that was carefully heighten'd by little and little, till the retort became ignited. And yet not the leaſt grain of mercury appear'd either in the receiver, or neck of the retort, at the bottom of which was found a very black light maſs like a powder, and of a very acrid, alcaline taſte. On the 28th of *November* he put it on a glaſs diſh in a cellar, where immediately it became moiſt; and there he left it till the 8th of *January* 1735; at which time it was increaſed in bulk, all the ſaline part having run ſpontaneouſly into a liquor, by means of the moiſt air, the metallic part remaining at the bottom under the form of a black powder. The whole was dried at the ſame time both what had run and what had not, and this mixture was found to be very black. He again put it into a glaſs retort, and towards the end he urged it with a fire, which ignited the whole for four hours; and there appeared not the leaſt ſign of mercury, either in the receiver or in the retort, at the bottom of which remained a matter of a cineritious colour and cauſtic taſte like fire, which expoſed to the open air immediately runs into a liquor.

In this proceſs, lead diſſolv'd with pure vinegar, and diſpos'd in ſuch manner as to be intimately penetrated by the ſalt; mixt and ground with a fixt cauſtic, liquid, alcali; expoſed to digeſt; ſet to putrify, brought to a ſtrong fire, and made to run by the moiſture of the air for a philoſophical month: and again ground, dried, and urged with a ſtrong degree of fire yielded no mercury at all.

4. After the Dr. had been aſſured from his own experience, that the ſalts, called *reſuſcitating ſalts*, could not, in the manner deſcrib'd, extract mercury from lead, he had a mind to try, what mercury itſelf could produce in this caſe; eſpecially ſince chemiſts call this fluid, *the water of metals*, in which ſay they, they die, revive, and become more beautiful, than they were before. He therefore pour'd an ounce of lead into a clean iron ladle; and at the ſame time he heated in ſuch another ladle three ounces of pure mercury; he afterwards pour'd the heated mercury to the melted lead; and they were immediately mixt together,

and formed a ſolid maſs of a ſilver colour: He ground this maſs, and after he had made it ſoft again, he heated it in a ſmall phial, which he afterwards ſtopt with a cork, and put in a digeſting heat, kept up always to an equable degree, *viz.* 84 degrees, from the 11th of *February*, 1732, till the 10th of *January*, 1735: It became a ſoft amalgam, yielding like butter to the peſtle, immediately growing black upon ſhaking it, and weighing 4 ounces. The ſame day he expos'd it in a clean glaſs retort to a ſand heat, and at length to ſo intenſe a fire of ſuppreſſion, that all the ſand was ignited for 4 hours; there came over into the receiver 2 ounces 6 drachms and ½ of mercury. The red powder formed by the mercury in diſtillation at the bottom and neck of the retort, and what little mercury ſtuck to the neck, and the few globules of pure lead appearing like a powder, weigh'd together 52 grains. In fine, there was at the bottom a ſolid maſs of lead, that weigh'd an ounce, wanting 5 grains, which compenſated the weight of thoſe ſmall globules of lead, juſt mention'd, that appeared like a powder: Whence it is evident, that the whole quantity of lead remain'd, and that 43 grains of mercury were diſſipated. Such as have any knowledge of theſe matters will readily find out the cauſe of this diſſipation in the reaſons above-mentioned; eſpecially when they conſider that a part of this mercury does in diſtillation adhere to the ſurface of a capacious, large receiver; and that the other part does in the form of *nubeculæ*, float on the ſurface of the water, which muſt always be pour'd into the receiver.

By this proceſs the Dr. learn'd, that no mercury can be extracted from lead, by a continu'd digeſtion of both for 3 years, nor by the ſtrongeſt diſtillation; nor that mercury by this means can be fixt into lead: For, in diſtilling the mercury, a ſmall quantity thereof is always changed into a red powder, which is fixed in the fire here made uſe of; but the weight of the lead always remain'd the ſame.

5. He made the ſame proceſs with an amalgam of 3 ounces of mercury and one of good tin; which he expos'd to the ſame degree of heat for the ſame time; and afterwards diſtill'd in the ſame manner in a glaſs retort with the ſame fire: The ſucceſs was, that from the receiver he had 2 ounces and 4 drachms of mercury: At the bottom of the retort was a powder, one part of which was fine, and conſiſted of a little fixed mercury; the other, coarſer, black, and compos'd of minute particles like tin. Nay, a little mercury ſtill adher'd to the neck of the retort,

retort, which in all weigh'd 2 drachms and 5 grains. At the bottom was a ſolid maſs of tin weighing 1 ounce 1 drachm and 9 grains: There was a waſte of 46 grains, the reaſons of which have been already aſſign'd.

It appears by this proceſs, that mercury cannot be extracted from tin; but 3 drachms with 14 grains (that is, better than a ſeventh part of mercury) were united with the tin, and ſo well fixed, that they could not be ſeparated therefrom by a continued fire of 4 hours, whereby the ſand was ignited. In *Nov. lumin. Chem. tractat.* IX. it is ſaid; 'but you cannot be ignorant of 'the great affinity between *Saturn* and *Luna*, in the middle 'of which is placed *Sol*; as alſo between *Jupiter* and *Mer-* 'cury, in the middle of which is likewiſe placed *Sol*.'

6. He pour'd 10 ounces of mercury, after heating it well, to 2 ounces of the beſt tin, melted in a clean iron ladle. He ground the whole into an uniform amalgam, which he put very warm and dry into a clean and warm glaſs bottle, which he afterwards ſtopt up tight, and put into a wooden box; this he tied to the ſtamper of a fulling mill, that was continually going almoſt day and night from the 30th of *Nov.* 1732, till the 9th of *January*, 1735. Then he took away the bottle, which was entire, and at the bottom he found running mercury; and after it had ſtood for ſome days, there was found in the upper part a ſomewhat hard amalgam; it weigh'd in all 12 ounces: He diſtill'd 11 ounces, 7 drachms of this amalgam in a luted glaſs retort with a naked fire, ſtill heighten'd to the end, and till the retort was quite ignited for 2 hours. No greater quantity of mercury came over than was put in, which was very fluid, and at the bottom remain'd a maſs of tin fixed to the glaſs, with a little dirty matter, foliated, as it were. This maſs was fuſible, like tin, in a moderate fire; and then its ſurface, expos'd to the air, was tinged with various colours. The maſs of tin weigh'd 1 ounce, 6 drachms and $\frac{1}{2}$; and there ſtill remain'd a ſmall quantity of the dirty matter already mention'd. It is, therefore, certain, that by a continu'd motion, mercury cannot diſſolve tin in the manner mercury may be extracted by diſtillation by the intenſeſt fire.

Scholium. What the Dr. obſerved remarkable in theſe 3 laſt proceſſes was, that the mercury, ſeparated by diſtillation from lead or tin, was exceeding liquid; and that by ſhaking it in a clean white glazed earthen diſh, it would in a ſhort time ſtain its ſurface, and there leave an exceeding black ſpot, cloſely ſticking to it. As ſoon as the Dr. had wiped off this ſpot with clean

clean and very dry paper, there was formed another ſpot, and immediately at ſeveral times others. This made him apt to think, that it was owing to the unctuous part of the metal, which in diſtillation paſſed with the mercury, and was then ſeparated from the ſurface to which it adher'd. In order to be aſſured of this, he ſpread the mercury upon very clean and dry white paper, where it left a ſlight black tract, wherever it paſs'd; the ſurface of this mercury was otherwiſe always cover'd with a very thin pellicle, that appear'd like fat: Therefore, tho' ſome particles of other metals may by repeated diſtillations of the mercury be united therewith, it will not follow from hence, that any particles are turned into mercury. He tried the ſame experiment with lead, that had been ſhaken in the ſame manner and for the ſame time: But when he was taking the bottle from the ſtamper of the fulling mill it happen'd to break, and the matter being loſt, he could not finiſh the proceſs.

Theſe experiments may ſerve to throw ſome more light upon the nature of mercury.

A partial Eclipſe *of the* Moon *obſerved at* Wittemberg Oct. 2. 1735. N. S. *by M.* Weidler. Phil. Tranſ. N° 443. p. 359. *Tranſlated from the* Latin.

H.	M.	S. *a. m.* Europ. time.	
0	44	30	The *penumbra* near *Schickardus*.
	59	0	The beginning of the eclipſe.
1	1	30	The ſhadow comes to *Schickardus*: Its edge rough and unequal. A little after, the moon is overcaſt.
1	15	0	*Tycho* entirely immerged. The moon immediately again overcaſt.
1	25	30	The obſcur'd portion of the moon grows black; nor can the *maculæ* be diſcern'd thro' the ſhadow with a 9 foot teleſcope.
1	30	0	The ſhadow comes to *Grimaldus*: now the ſpots are ſeen thro' the ſhadow.
1	44	30	The ſhadow covers *Grimaldus* entirely: Now the darkened portion is reddiſh. And immediately the moon is again overcaſt.
2	25	30	The ſhadow receding touches *Lansbergius*. Its edge is ſtill rough.
2	44	0	The ſhadow comes to *Gaſſendus*.
3	11	0	*Tycho* begins to emerge.

H.

H. M. S.
3 36 0 The end of the eclipſe above *Snellius*, the ſky clear about the moon.

A ſhock of an Earthquake *felt in* Suſſex *on the 25th of* October, 1734; *by the* Duke *of* Richmond. Phil. Tranſ. N° 444. p. 361.

ON the 25th of *October*, 1734, between 3 and 4 o'clock in the morning, there happen'd an extraordinary earthquake in *Suſſex*: And what confirms his Grace in the opinion that there really was an earthquake, is, that almoſt every body agree in the ſame deſcription, as to the ſenſation, the hour of its happening, and the perfect calm that was at that time. His Grace obſerves that the ſhock was vaſtly more felt towards the ſea-ſide as at *Shoreham*, *Tarring*, *Goreing*, *Arundel* and *Havant*. At his Grace's houſe of *Goodwood*, near 3 miles north of *Chicheſter*, and about 7 from the ſea, it was not ſo perceivable as at *Chicheſter*, where it was ſtill leſs ſo than by the ſea-ſide. His Grace had not heard that there was the leaſt touch of it felt in any parts of the vale on the north ſide of the *Downs*, which for the moſt part run eaſt and weſt.

A farther Account of the ſame Earthquake *at* Havant *in* Suſſex Oct. 25, 1734; *by Dr.* Edward Bayley. Phil. Tranſ. N° 444. p. 362.

OCT. 25, 1734, between 3 and 4 o'clock in the morning, an earthquake was felt at *Havant* in *Suſſex*: The ſhock was ſo conſiderable, as to be obſerved by one or other in moſt houſes of the town. The Dr. happen'd to be awake at that time, and he perceiv'd the bed ſhake under him with a quick tremulous motion, which continu'd about 2 or 3 ſeconds, and then ceas'd; and after a very ſhort intermiſſion was repeated in the ſame manner, and laſted about the ſame ſpace of time, as near as he could gueſs. He was at firſt greatly ſurpriſed at ſo unuſual a phenomenon; but upon a little recollection, he concluded it muſt be occaſion'd by an earthquake; and he was ſoon confirmed in his conjecture by the concurrent obſervations of his neighbours, and afterwards by accounts of the ſame from ſeveral other places; in ſome of which it ſeemed to have been more violent than at *Havant*. Several perſons in this laſt place affirm, they not only perceiv'd the ſhaking of their beds, but alſo the rocking of their houſes, together with a rumbling noiſe of drawers, and the like moveable goods in their chambers and other

other rooms. A learned and ingenious Gentleman at *Havana* informed the Dr. that the motion of his bed appear'd to him like the toſſing of a veſſel when it croſſes over a wave, the head and feet thereof riſing and falling alternately ſeveral times; whereas the Dr's ſeemed rather to rock from ſide to ſide: But theſe contrary motions of the 2 beds are eaſily accounted for, by conſidering the different poſitions of them, his friend's bed ſtanding directly eaſt and weſt, and the Dr's north and ſouth: For, ſuppoſing the undulatory motion, which the earth might have at that time, to be propagated from eaſt to weſt, the ſame kind of motion which caus'd his friend's bed to riſe up and down lengthways, muſt make the Dr's rock from ſide to ſide; as may be obſerved in 2 veſſels ſailing in contrary directions on the ſame waves of the ſea; that which croſſes the waves at right angles being toſſed up and down end-ways; while the other, moving in a line parallel with the waves, will be rocked from ſide to ſide. What makes the Dr. the more inclined to think the progreſſive motion of this earthquake to have been from eaſt to weſt, is, becauſe it appears from the beſt accounts he had of it, that it was obſerved ſooner eaſt than weſtward, and likewiſe extended farther from eaſt to weſt than north and ſouth.

He thinks it may not be amiſs to take notice of ſome remarkable phenomena, which happen'd before and after, as well as ſome other circumſtances, which immediately attended this earthquake; moſt of them agreeing with thoſe ſigns, which have been obſerv'd by the learned to precede or accompany former earthquakes in theſe and other parts of the world. It is obſervable, that there was more rain and wind for ſeveral months ſucceſſively, than for many years before; eſpecially from the beginning to the middle of this month, about which time it clear'd up, and the weather became ſuddenly very cold with froſty mornings, the wind blowing generally pretty hard from N. W. On the 23d of *Oct.* the cold abated conſiderably; it was cloudy, but no rain that day. The 24th was calm all day; it rain'd moſt part of the afternoon, tho' the mercury ſtood at 30 $\frac{2}{10}$. It continued very calm all night, and rain'd hard for ſome time before and after the earthquake happen'd; but it ſoon clear'd up, and a ſtrong gale of wind, aroſe within half an hour, or as ſome ſaid, within a quarter afterwards: It continued blowing hard all the forenoon. At 4 o'clock in the morning the Dr. obſerved the mercury continu'd at 30 inches $\frac{2}{10}$; the ſpirit of wine at 55 $\frac{1}{2}$, having riſen about 5 degrees ſince the late cold weather.

N. B.

N. B. The Dr's barometer and thermometer were both in one frame, and made by Mr. *Hauksbee*.

The circumſtances related by his Grace the Duke of *Richmond* and by Dr. *Bayley* are confirmed by the united teſtimonies of ſeveral perſons of veracity, who ſigned certificates of what they obſerved concerning this accident at *Chicheſter* and other places.

Mr. *Boisdune* of the pariſh of *Funtington* in the county of *Suſſex* and ſeveral perſons living in the city of *Chicheſter*, all agree that there was a manifeſt ſhock of an earthquake felt on *Oct.* 25 about a quarter before 4 in the morning, which laſted by fits ſome few ſeconds, about a quarter of a minute, or while one might tell 20, with a motion ſenſibly ſlow: For, moſt of the accounts concur in this particular, namely, that the chairs, wainſcot, doors, cheſts of drawers and other moveables, were heard rattling; and one, that a bell rung of itſelf juſt before they felt the heaving of their beds; and that there was no wind ſtirring at that time, but that it rain'd, and the wind roſe ſoon after.

Mr. *Green*, prebendary of *Chicheſter*, had informations of the ſame tremblings, attended with the ſame circumſtances, being felt at *Shoreham*, *Goreing*, *Tarring*, *Findon*, *Arundel-caſtle* and *Merſton*.

John Shaw, *Thomas Dagly*, and *John Towner*, all ſervants to the Duke of *Richmond*, felt the ſame at his ſeat, call'd *Goodwood*.

Mr. *Jenkins*, riding officer of the cuſtoms in the pariſh of *Weſt-Wittering*, near *Bragleſhambay*, in the county of *Suſſex*, deſcribed the ſhock after the ſame manner: And he farther adds, that within half a quarter of a mile of his houſe, a young man, about 18 or 20 years of age, having been at the ſame time to fetch up a team of horſes from graſs, the horſes were ſo ſenſible of ſomething more than ordinary, that they ſtamped and ſeem'd very much affrighted, as they were coming home.

Of a ſhock of an Earthquake, *felt at* Aynho *in* Northamptonſhire *October* 10. 1731; *by Mr.* Joſ. Waſſe. Phil. Tranſ. N° 444. p. 367.

ABOUT 4 in the morning *Oct.* 10. 1731, Mr. *Waſſe*, Rector of *Aynho* in *Northamptonſhire*, ſays, that his windows rattled, as if ſome body had been dancing over head: The concuſſion laſted about a minute; others thought it laſted

about two minutes: It alarm'd the neighbouring villages *Bloxam*, four miles south-west from *Aynho*; *Barford*, five: *Banbury* four miles west; *Aderbury*, a mile west: *Crowton*, a mile to the east; and *Charleton* as much to the north. There was no notice taken of its progress south or south-east..

About a minute after, some of the town of *Aynho* saw a great flash of lightning. In the morning the sky looked of a land-colour: It was said that there was a former shock felt *October* 8, about three in the morning; and that the latter was preceded by a noise like distant thunder.

It is remarkable, that this shock at *Aynho* was perceiv'd to extend more from east to west, than from north to south, which particular was likewise observ'd in the shock felt in *Sussex*, in 1734.

The sequel of the Experiments on Mercury; *by Dr.* Boerhaave. Phil. Trans. N° 444. p. 368. *Translated from the* Latin.

FROM the observations on mercury, *Phil. Trans.* N° 430, p. 145, and N° 443, p. 343. it appeared, that the nature of mercury is never changed, tho' surprisingly varied in appearance it should often seem to change into new forms. The Dr. now comes to rehearse some other processes he had made; whence a much greater constancy or immutability is evinced, and at the same time the nature of other metals will be shewn. The most ancient Alchemists, and who may justly be reckon'd the best, unanimously agree, that mercury is quick metal: But then at the same time they affirm, that when it is genuine and free of all impurities, it is so simple, as to be entirely the same in all its parts, and incapable of being divided into parts of a various nature: They moreover affirm, that upon that account it is immutable to every cause, and at the same time of so penetrating a nature, as to be able to dissolve all other things; and yet itself remain unchangable: And as they are plainly agreed in these things, so likewise in this, that it is never got pure from its veins, but always tainted with a heterogeneous impurity, which, originally being concreted and hence growing up with it in a surprising manner, is intimately incorporated therewith. They regret that it is with the greatest labour it is freed of that impurity; because in the primeval formation of the seed it is insinuated and almost indissolubly incorporated with its very principles of vegetation; and this impu-

impurity they call'd ſulphur; in reſpect of which alone they diſcover'd quick-ſilver mutable; and this alone they blame as what hinders its ſubtile penetrating quality by obtruding the peculiar edge of mercury; and what makes it unite with adventitious matter. But ſhould an ingenious and lucky artiſt by ſome ſecret method free this mercury from all its congenial impurity, it would be no longer changeable, it would acquire a degree of ſubtility, by which it would pervade all other things; would not mix with any thing in nature, but obſtinately retain the ſtricteſt purity. But how ſurpriſing is its nature! It concocts crude metals, perfects the baſer ones, readily attenuates and reſolves all other bodies, and turns them into their radical moiſture; and thus it is juſtly cried up as the chief inſtrument in medical and chemical arcana's. The adepts, it is true, tell us, that it reſembles fire, which remaining unchangeable itſelf, changes every thing elſe: and dividing and uniting every other thing, remains itſelf untouch'd and intirely free.

By ſuch tempting promiſes chemiſts were incited to find out a method of freeing mercury from this impurity, in order to have it pure and ſimple: And the wiſeſt of them thought it was to be burnt out by fire alone; becauſe fire is the ſole purifier of metals: Hence aroſe the diſtillation of mercury in clean cloſe glaſſes; and this ſo often repeated, till the whole was turn'd into a red, ſhining powder. But urging this powder with the ſtrongeſt degree of fire in clean glaſſes, they again recover their priſtine mercury; which they miſtakenly took for purified, ſince by the new action of fire it ſuffers itſelf to be again reduced into a ſimilar red, and ſhining powder. And the greateſt maſters openly deny that quick-ſilver duly purified and defecated by art, can ever by the action of fire be brought into a powder, even if the proceſs be continued to eternity. And here perhaps it may be proper to peruſe what has been ſaid *Phil. Tranſ.* N° 430, about 5 or 6 diſtillations of the ſame mercury in glaſs veſſels.

But it is ſcarce credible, that its depuration can be obtain'd by ſuch a method; and poſſibly there was another method of working by which this was to be done: And the chief Maſters in the art do plainly enjoin a different method: For, they ſay that this depuration is procured, while the nucleus of the mercury is detained by the pureſt bodies, which from their near affinity with it, do cloſely and indiſſolubly

lubly unite therewith: But gold and silver are pure, fixed metals, and very like genuine mercury, either if we regard their origin or their matter. They hold that it hence follows if mercury be mixt with a perfect metal, and by means of fire be again separated therefrom in glass vessels close stopt, that the part of the pure metal will take in the mercury, and at the same time separate the impure from the pure part: The Dr. had a mind to try what truth was in this assertion, by setting about a laborious process, the success of which was as follows.

The Dr. had at the bank at *Amsterdam* two ounces and ½ of the purest gold, that could be procured by Assayers art; he divided it into five small masses that weigh'd ½ an ounce each: He put them all into a clean glass retort, and poured 25 ounces of pure mercury that had been once distilled before; half the mercury came over from the gold, which lay under it at the bottom of the retort. Having thus finished the process, there came over 13 ounces of mercury into the receiver: At the bottom of the vessel the gold was now entirely dissolv'd in the mercury, in the form of a perfect white mixt, call'd an amalgam: Whence it appears, that gold is dissolv'd by the heat alone of boiling mercury; and this seems to be the best method of mixing these two together, call'd by artists *Amalgamating*. After drying well the quicksilver that came over, he return'd it upon the remainder in the retort; and he again brought over by the fire an equal quantity of mercury, which after drying he poured afresh upon the remainder: And this he repeated 50 times. At last there came over pure mercury: In a glass mortar he ground with a glass pestle the black amalgam that remain'd at the bottom of the retort, and the water he poured thereon became muddy, he wash'd it with pure water, which again became muddy upon grinding. This he caused to do for 13 days, when the water was no longer muddy, but the amalgam shone bright, and the water remain'd clear. The powder, prepar'd by grinding and washing, which was of a dark colour, and of a horrid metallic taste, when dried well, weigh'd 83 grains; the mercury and gold weigh'd 26 ounces, 7 drachms; there were seven grains lost; three drachms and ½ in 50 processes: This happen'd partly by the mercury evaporating, and partly by its adhering to the filtring paper, by which it was dried from the water, into which it was receiv'd in distillation.

He

He again treated this pure amalgam in the ſame manner other 50 times: And now the 50th time there came over pure mercury: At the bottom of the veſſel there remained a dark amalgam; which after triture and lotion with water, in the manner above mentioned, for 13 days together, yielded 1 drachm, 44 grains of a clean, dark, dried powder: And then the pure amalgam, together with the mercury, weigh'd 26 ounces, 4 drachms: By theſe 50 operations he loſt 1 drachm, 16 grains.

He again caus'd diſtil for 50 times in the ſame manner this depurated amalgam: And there came over pure mercury; and at the bottom of the retort there was a reddiſh amalgam, which ground and waſhed with water as before, for 14 days, yielded 1 drachm, 11 grains of a dark powder. He added the mercury that came over to the pure amalgam, which together weigh'd 26 ounces, five drachms 24 grains: But while he was pouring off the amalgam, there ſtuck ſome of it to the bottom of the retort; ſo that he could not compute the waſte.

The depurated amalgam again treated 50 times in the ſame manner, as by diſtillation, triture, and lotion for 14 days, yielded a dark powder, that weigh'd one drachm and ½ and four grains. The moſt ſhining amalgam, mixt with the pure mercury that came over, weigh'd together 25 ounces, two drachms, 46 grains after 200 diſtillations.

He urged this amalgam 50 times as before; and afterwards grinding it anew with water for 16 days together, he got two drachms, one ſcruple, and 4 grains of a dark powder; the bright white amalgam, together with the mercury, weigh'd 25 ounces, one drachm, 46 grains.

Having gone through this laborious proceſs he obſerv'd that after 250 diſtillations of mercury with gold, both of them yielded one ounce, five grains, of the above deſcribed powder; that there remained 25 ounces, one drachm, 46 grains of gold and mercury; and that there was loſt, in the manner mentioned, one ounce, two drachms, and 9 grains.

The attentive conſideration of theſe things made him with joy begin to ſuſpect, that he now ſaw the ſo much deſired method of purifying mercury, whoſe impurity probably lay concealed in that powder, which he ſometimes thought to be a mere fetid, dirty ſulphur, that ſtain'd the virgin purity of quickſilver, and he doubted whether now he did not ſee it pure, after having undergone the action of fire and water.

But

But his joy was check'd upon recollecting how often his over-haſty hopes had vaniſhed into ſmoke: Amidſt theſe doubts he reſolv'd not to reſt, till he was certainly aſſur'd of the truth; he, therefore, caus'd diſtil over again 25 ounces, 1 drachm, 46 grains of that laſt and pureſt amalgam, always bringing over half of the mercury, and pouring it on again for 627 ſeveral times; and he ceaſed waſhing it any longer with water, in order to ſee the event. By this proceſs the matter uſually grew dark, ſo as at length to be almoſt black: Then he did over the glaſs in which it ſtood with a coating that could bear an open fire; and thus he urged the black amalgam, that was no longer waſh'd, with ſo intenſe a fire, as that in 3 hours time the retort was entirely ignited; and there came over 20 ounces of the pureſt mercury; and afterwards breaking the veſſel he found at the bottom 2 ounces and $\frac{1}{2}$ of the brigheſt gold, without leaving any dreg at all behind.

He afterwards took 7 drachms, 57 grains of the powder of 250 diſtillations; and urged it with a very intenſe, open fire in a retort coated over, ſo as to ignite for ſome time: From this powder were recovered 7 drachms, 46 grains of the pureſt mercury; at the bottom of the retort remain'd, as he found, 6 grains of a dark powder.

Upon weighing hydroſtatically the quickſilver urged in this manner 877 times, he found it to pure water, as 13 and $\frac{1}{2}$ to 1; ſo that its denſity was not changed, notwithſtanding it underwent ſuch a variety of operations, nor was it freed of any of its lighter parts. As the Dr. underſtood that his method of finding out the weights, mention'd *Phil. Tranſ.* N° 430. was ſuſpected not to be ſufficiently accurate, he now adds what follows from what was ſaid above.

1. Gold diſſolved by mercury, by repeated coction and triture, changed nothing of its priſtine nature, loſt nothing of its proper weight, and acquir'd nothing, as far as could be gather'd from the foregoing experiments.

2. Quickſilver, mixt with gold, and again ſeparated from it by fire, has part of it turned into a dark, ſubtile, powder, of a horrid metallic, taſte, and of a quite different nature from what it was before; and this always happens till 877 times; yet it again returns, by a ſtronger degree of fire alone, to the ſame quickſilver it was before, with every quality obſervable by art.

3. Fire therefore and gold do not by this means ſeparate from quickſilver different parts, as ſulphur, dregs, or any other thing;

thing; but only change it in outward appearance, which is again recoverable, and ſo entirely the ſame, that its proper weight is in nowiſe changed.

4. Quickſilver and gold by the force of fire do immediately change the ſilver brightneſs of their amalgam into a dark and at length a black colour: But the reſtoring the ſilver brightneſs to the mercury, and the yellow brightneſs to the gold by a ſtronger degree of fire alone, ſhews that this colour does not infer any corruption or real change in the nature of the metals.

5. Yet if native mercury can poſſibly be depurated by gold and fire, according to the opinion of the ancients, this muſt be perform'd by ſome other proceſs.

6. The hopes of fixing mercury with gold by the action of fire prove all abortive: Since after ſo much labour and length of time, there is not even a beginning towards it; the laſt diſtillation was performed as eaſily as the firſt.

7. Hence we have no confirmation of the opinion, that fire can concrete to metals or to mercury to the increaſe or generation of ſomething metallic; or to the fixt ſtable change of the metal itſelf.

8. Great then is the conſtancy and ſimplicity of quickſilver and gold! If gold were originally quickſilver; is it not then truly ſaid, that mercury either entirely evaporates in the fire, or remains entirely fixed therein.

9. The big promiſes of 2 great men in the profeſſion about the diſſolving of gold by triture, either with or without water, prove all vain: They declin'd the troubleſome taſk, and were over haſty in making concluſions.

There remain'd one thing worth enquiry; whether mercury, diſtill'd ſo often from gold by the force of fire, had not depoſited that property, by which it is turned by diſtillation into the powder, call'd precipitate *per ſe?* He, therefore, caus'd diſtil in a clean glaſs retort theſe 20 ounces of quickſilver, diſtill'd 877 times from gold, and that with ſo ſtrong a fire, that no mercury at all remain'd in the retort after each diſtillation, and this he repeated 8 times. At the bottom of the retort he had 12 grains of a ruddy, ſparkling, ponderous, mercurial precipitate, of a horrid metallic taſte. The Dr. therefore was aſſur'd, that even this quality is not taken from the mercury by this laborious proceſs.

An

An improvement of the Diving Bell; *by M.* Triewald. Phil. Tranſ. N° 444. p. 377.

M. *Triewald* having made trials with the diving bell and air barrels in ſeveral depths on the coaſt of the *Baltic*, according to the ingenious improvement of Dr. *Halley*, made in 1716, but with ſome ſmall additions, found by experience, that no invention built upon any other principles than thoſe of the *campana urinatoria*, can be of uſe in any conſiderable depths; or that the diver in any other invention whatever, can be a ſingle moment ſafe. As to the many inconveniencies that attend other inventions, he only mentions that of a water armour, in which the man is drowned in an inſtant, when ſuch a machine receives the leaſt leak: Whereas experience has ſhewn, that the danger is not ſo great when ſuch an accident happens to the diving bell; as to his knowledge it did once, when the diver was 12 fathom under water, and a pretty large hole was ſtruck into the bell, by a boult of the wreck he went upon; when the air ruſh'd out of the ſame with ſuch violence as aſtoniſh'd the beholders by the exceſſive boiling on the ſurface of the water, but the man in the bell clapp'd his hand to the hole or leak, and gave a ſignal to be haul'd up; which was done with all the eaſe and ſafety, as if no ſuch accident had happen'd, the water having only riſen about half a foot into the bell by this leak.

This ſame man had a worſe accident befal him at another time, by the bell's ruſhing down at once about a fathom or more, by the careleſsneſs of thoſe that worked it; the blood guſhing out of his noſe and ears, he felt an intolerable preſſure on his whole body; which ſhews, that when a man in a diving bell is ſlowly and gradually let down, he at ſuch a time and by degrees reſpiring compreſs'd air, which by the lungs is forced into the blood, cannot feel the external preſſure of highly compreſs'd air, nor of the water reaching ſome parts of his body; a conveniency no other invention can afford, where the diver is to draw his breath from air in its natural ſtate.

M. *Triewald* has often with pleaſure obſerv'd, that when he has caus'd the bell to ſtop, at 5 fathom depth, and the diver taking in the air contain'd in an air barrel, lower'd down a fathom deeper than the bell, without opening the cock for diſcharging the hot air; the water would, by the acceſs of the air out of the barrel, be entirely, or to a very ſmall matter, expell'd out of the bell; and when the ſame was again lower'd down

lown 5 fathom more, the ſame operation with another air barrel repeated, and the bell afterwards haul'd up, it was no ſmall matter of delight to obſerve, that every fathom the bell came up, it would diſcharge itſelf of the ſuperfluous air in a large quantity, which came up from the bottom of the bell, in bubbles, as big as oſtrich eggs; and thus diſcharge of air continu'd, til the equilibrium of the air in the bell, and preſſure of the water, were reſtor'd, and till the bell came above the ſurface of the water.

At other times he obſerved, when no air was by the way taken into the bell, but the ſame lower'd down the common way, and haul'd up again after ſome time, that the very inſtant when the bell ſhould part with the ſurface of the water, the ſtrength of two men more was requir'd at the capſtan at that time, than before and after the bell hung freely in the air: From whence he preſumes it plainly appears, that the air, which paſſes thro' the lungs of a living creature, loſes its elaſticity; and that the lungs of a man make a kind of a *vacuum* in the bell; for which reaſon the diver feels at the very inſtant, when the bell parts with the water, a very ſmart preſſure in his ears.

Tho' experience has thus taught, that no invention is more ſafe and uſeful than the *campana urinatoria*, with the ingenious improvements of Dr. *Halley*; yet M. *Triewald* has likewiſe found, that this invention is not to be made uſe of without conſiderable charge; it requiring a large veſſel, and number of hands, to work and manage ſuch a large diving bell, together with the air barrels and their reſpective weights for ſinking: That the expences therefore might be leſſen'd; and that the diving bell, nevertheleſs might anſwer all the intents and purpoſes of Dr. *Halley*'s, M. *Triewald* has made the following improvement on it.

The diving bell A B (Fig. 2. Plate VIII.) M. *Triewald* caus'd to be made of copper, and he reduced the ſame to a very ſmall compaſs in regard to Dr. *Halley*'s, as may be ſeen by the ſcale under the draught; by which means it is eaſily managed by 2 hands: Yet he preſumes that a diver may live in the ſame for as long a time, and with as much eaſe, at a very conſiderable depth of water, as in a bell of twice its capacity; becauſe that tho' a man in a large bell has undoubtedly more air than in a leſs; and conſequently, ſhould be able to ſubſiſt a great while longer on a large than on a ſmall quantity of air; yet ſince his head is generally in the upper part of the bell, where the hot air takes up its reſidence, he receives very little or no

benefit of the air under his chin or breaſt, tho' never ſo fit for reſpiration; which air, nevertheleſs, in the lower parts of the bell will remain cool a long time after he has been in the bell, and has with difficulty drawn his breath; as is very obvious to any one who has been in a *German* Bagnio, and ſuch as are uſed in *Sweden*; where in a ſingle room all the degrees of heat are to be felt, by means of a contrivance like ſtairs to the very top of the cieling: A man, when he places himſelf on the uppermoſt ſtep, will feel an exceſſive heat; ſo that any body not very much us'd to it cannot endure the ſame, nor draw his breath, but faint away: Whereas on the firſt, ſecond, and third ſteps from the floor, the heat is very moderate; nay, ſometimes the air near the floor is pretty cool; when at the ſame time near the cieling the heat is intolerable.

To obviate this inconveniency he caus'd a ſpiral tube of copper, *b*, *c*, to be placed cloſe to the inſide of the bell, and fixt in ſuch manner that the ſame may be taken out and cleans'd at pleaſure, and with eaſe; and at the ſame time not encumber the diver, when he is in the bell: At the upper end of this tube *b*, a flexible leather tube is join'd 2 foot long, at the end of which is a turned ivory mouth-piece, which the diver (as ſoon as he perceives the air to grow hot in the top of the bell) keeps conſtantly in his mouth; which may be done by means of the flexible tube in whatever poſture he is in, as ſtanding, ſitting, bending his head, *&c.* and all the while he draws his breath thro' the abovementioned tube, and the air from *c*; by which contrivance he not only draws continually cool and freſh air as long as any is in the bell, but occaſions at the ſame time a circulation, which is ſo neceſſary to the very being of air (eſpecially in a compreſs'd ſtate) and its preſervation for the uſe of animals; a thing he has found of great conſequence; and ſo much the more neceſſary, as any body who has been in a diving bell for a long time, without any new ſupplies of air, and has been reduced to the laſt extremity of breathing in the ſame, will agree, that when at ſuch a time the bell begins to be haul'd up; and by that means the compreſſed air is allow'd to expand and is put into motion never ſo little, the man receives a new life, as it were, and incredible comfort and eaſe.

Again, when in coalpits, levels are drove in the coal or thro' dykes, the air of the level or adits growing hot by the breath and ſweat of the hewers and workmen for the want of a circulation of the air; M. *Triewald* has found it to be an excellent remedy, to place, along the ſide of the drift or adit, a ſquare wooden

wooden box, open at both ends, laid from the place where the air is cool and good, and reaching, by joining one box close to another, as far as where the work is carried on. And thus by this simple contrivance, a circulation of air is obtain'd, and sometimes to that degree, that when a candle is held at the end of the box, where the cool air enters, the flame is driven out by the current of cold air entering and circulating thro' the box.

By this experiment he is apt to think, that tho' the diver should not keep the end of the flexible tube in his mouth, which he may do with all the ease in the world, yet that the air would circulate thro' the copper tube, and he receive no small benefit by it: DDDD are the weights for sinking the bell, contriv'd in such manner as with great ease to be hook'd on the same hanging on the cable. The iron plate E, fixt to the chains F F F, serves the diver to stand upon, when he is at work.

The bell is very well tinn'd on the inside; and as in all rivers, and the coasts of the *Baltic*, the water is exceeding clear and limpid; because there is no ebb and flood, M. *Triewald* has placed 3 strong convex *lens*'s G G G: By these means the diver can not only see what is under him, but likewise on all sides at a good distance.

These glasses have strong copper lids, HHHH, like snuff boxes; which lids are shut, when there is no occasion to discover any objects in the bottom of the sea, and serve to preserve the glasses from being broken.

A Description of the Moose-deer *of* New England, *and a sort of* Stag *in* Virginia; *together with some Remarks on Mr.* Ray's *Description of the flying* Squirrel *of* America; *by Mr.* Samuel Dale. Phil. Trans. N° 444. p. 384.

THE moose-deer has been mention'd by several authors; but their accounts have generally been so very imperfect that little satisfaction hath thereby been given to the curious enquirers into natural history. The first mention that Mr. *Dale* finds made of this animal is by Mr. *Josselyn*, in a little tract, call'd *New England's rarities*; where p. 19. he writes, 'that 'it is a goodly creature, some of which being 12 foot high, 'their horns exceeding fair, with broad palms; some being '2 fathoms from the top of one horn to the other.' Much to the same purpose is the account he gives of this animal in another book of his, called 2 *voyages to New England*, where p. 88 he says, 'that the moose or elke is a creature, or rather a

'monſter of ſuperfluity, when full grown, being many times 'bigger than an *Engliſh* ox.' What *Neal*, in his *Hiſtory of New England* Vol. II. p. 573, hath of this animal, call'd by him the *moſe*, is copied from the aforeſaid *Joſſelyn*. The beſt and fulleſt account of this animal was ſent by Mr. *Dudley*; and publiſh'd *Phil. Tranſ.* N° 368. p. 165, where he makes them to be of 2 ſorts, *viz.* the common light grey mooſe, call'd by the *Indians*, *Wampooſe*; and the large or black mooſe. As to the grey mooſe, Mr. *Dale* takes it to be no other than that Mr. *John Clayton*, in his account of the *Virginia* quadrupeds, publiſh'd in *Phil. Tranſ.* N° 210. p. 122, calls the elke; which in the *Memoirs for a Natural Hiſtory of animals*, publiſh'd at *Paris* and engliſh'd by Mr. *Pitfield*, p. 167, is call'd by the name of the ſtag of *Canada*, of which Mr. *Dale* has ſeen a ſingle horn, ſent by Mr. *Mark Catesby* from *Virginia*, by the name of an elke's horn, and was in all reſpects like thoſe of our red deer or ſtags, only larger, weighing about 12 pounds averdupois; and from the burr to the tip meaſur'd by a ſtring about 6 foot high. Mr. *Dudley* writes that his grey mooſe is likeſt the ordinary deer; that they ſpring like them, and herd together ſometimes to the number of 30 in a company: But whether he means the red, the *Virginian*, or the fallow deer, is uncertain, he having ſaid nothing of their hours, which was neceſſary to diſtinguiſh them. The black mooſe is accounted by all, that have hitherto writ of it, a very large creature. The above-mention'd Mr. *Joſſelyn* makes it many times bigger than an ox; and Mr. *Dudley* writes, that the hunters have found a buck or ſtag-mooſe 14 ſpans high from the withers; which at 9 inches to the ſpan, is 10 foot and ½; and that a doe or hind of the fourth year, kill'd by a Gentleman near *Boſton*, wanted but one inch of 7 foot in height. The ſtag, buck, or male of this kind hath a palmed horn, not like that of our common or fallow deer, but the palm is much longer, and more like that of the *German* elke; from which it differs, in that the mooſe hath a branched brow-antler between the burr and the palm, which the *German* elke hath not.

Fig. 3. Plate VIII. repreſents the head, or rather the attire (as it is called in heraldry) of a black mooſe-deer, which was ſent Mr. *Dale* from *New England*; the dimenſions of which are, as follows:

A	B	56 inches
C	A	34
C	E	31
C	D	34
D	H	30
F	G	9 ½
F	I	14
K	L	7

The horn of this *New-England* black mooſe agrees not in figure with either of thoſe mentioned in *Phil. Tranſ.* N° 227. p. 489. and N° 394. p. 123, found foſſil in *Ireland*; the laſt of which Mr. *Kelley* writes, that for want of another name, they call'd them elks-horns. Mr. *Dale* ſuſpects that thoſe horns Mr. *Ray* mentions in his *Synopſis methodica animalium quadruped.* to have ſeen with Mr. *Holney*, an apothecary at *Lewis* in *Suſſex*, as alſo in divers *Muſeums*, were not the horns of this black or *American* mooſe, but of the *German* elke; becauſe that inquiſitive gentleman takes no notice of any brow-antlers they had, which Mr. *Dale* thinks was too notorious to have eſcaped his obſervation had there been any ſuch.

As to the number of young ones, or calves, which the mooſe brings forth at a time, authors vary: For, Mr. *Dudley* ſaith, that they bring forth but two: But *Joſſelyn* in his *two Voyages*, p. 89; and from him *Neal*, that they bring forth three; and that they do not go ſo long pregnant, as our hinds, by two months. What theſe two laſt mentioned authors write as to their caſting their calves a mile diſtant from each other, doth not ſeem probable; nor does Mr. *Dale* find, that *Neal*, in his deſcription of this creature, makes any mention of their having a long tail, tho' charged ſo by Mr. *Dudley*, who likewiſe omits the brow-antlers in his deſcription of their horns.

There is another beaſt of the deer kind, which though very common in *Virginia*, and undoubtedly, in other of the northern provinces of *America*, yet ſo far as Mr. *Dale* knows is not deſcribed by any author. Mr. *Beverly* in his *preſent ſtate of Virginia* mentions both elke and deer in that country, but doth not deſcribe either.

But by what Mr. *Dale* receiv'd from Mr. *Catesby*, the firſt ſhould be the *Canada* ſtag, and the other, the deer here mentioned. Mr. *Clayton* likewiſe mentions the elke, which

he

he ſaith are beyond the inhabited parts, and are the ſame with Mr. *Beverley*'s; as alſo the deer of which he ſaith there are abundance, yet he doth not deſcribe them, but calls them red deer, though they are not the ſame with what we here call by that name, but of thoſe that follow.

That which Mr. *Dale* takes for the undeſcribed deer, is of the ſtag-kind, having round horns like them, not ſpreading out as in the ſtag or red deer, but meeting nearer together at their tips, and bending forwards over the face of the animal; the brow-antlers are not crooked, ſtanding forwards, but ſtreight and upright, as repreſented Fig 4. the dimenſions of which are as follows.

a b 11 inches
a cb 20
a d 12 $\frac{1}{2}$
d f 12 $\frac{1}{2}$
d e 11
g h 2 $\frac{3}{4}$

The skin of this deer is of a ſand colour, with ſome black hairs intermix'd, and while young ſpotted all over with white ſpots, like ſome ſorts of fallow deer; being likewiſe about their ſize when full grown. The *Dama Virginiana Raii Synop. Animal. quadruped.* p. 86. which was formerly in St. *James's-Park*, ſeems to be different from this; if Mr. *Willoughby* was not led into a miſtake in taking it to be of the palmate kind, by only ſeeing it when the horns were ſhed: Perhaps this laſt of Mr. *Ray* may be the maurouſe of *Joſſelyn's Voyages*, p. 91. which he ſays is like the mooſe, only his horns are but ſmall, and the creature about the ſize of a ſtag; but his deſcription is too ſhort to be ſatisfactory.

There are other ſorts of deer mentioned by Mr. *Joſſelyn* in his laſt quoted book, p. 87. as natives of that country, as the buck, ſtag and rain-deer: But whether they are the ſame with thoſe call'd by the ſame names in *Europe*, Mr. *Dale* cannot determine; the deſcriptions of them being omitted. Mr. *Joſſelyn* likewiſe mentions as another ſort of *American* deer, an animal call'd a maccari, caribbo, or pohano: But by the account he gives, it ſeems to be a fiction; no ſuch animal being, Mr. *Dale* thinks, in *rerum natura*

Mr.

Mr. *Ray* in his Synop. quad. p. 215, rather refers the *Sciurus Amerioanus volans* to the mouſe than to the ſquirrel kind, becauſe their tails are broad aud plain, and not turn'd over their backs when they ſit; which miſtake may, probably, ariſe from only ſeeing the skin of one dead, when the hair of their tails had been eaten off by mites: For, in one Mr. *Dale* ſaw alive, which was brought over from *Virginia*, the tail was hairy, as in others of the ſquirrel kind, tho' rather thinner; and it turned over the back as in other ſquirrels.

Dr. *Mortimer* obſerves, that the ſame ſpecies of flying ſquirrels hath been found in *Poland*; a deſcription of which together with an accurate Fig. is given by M. *Klein*, *Phil. Tranſ.* N° 427.

And that as to the large horns found foſſile in *Ireland*; he has taken particular notice (in ſeveral he ſaw) beſides the main horns being palmated, that the brow-antlers are ſo likewiſe; a circumſtance peculiar to the rain-deer ſpecies, being of great ſervice to them in removing the ſnow, in order to get at the graſs or moſs underneath, which is their chief ſubſiſtence in *Lapland*.

An Attempt to explain the Phenomenon of the Horizontal Moon appearing bigger than when elevated ſeveral Degrees above the Horizon, ſupported by an Experiment; by Dr. Deſaguliers. Phil. Tranſ. N° 444. p. 390.

THIS apparent increaſe of the moon's diameter (which a teleſcope with a micrometer ſhews to be only apparent) is owing to the following early prejudice we have imbibed from children.

When we look at the sky towards the zenith, we imagine it to be much nearer to us, than when we look at it towards the horizon: So that it does not appear ſpherical; according to the vertical ſection E F G H I [Fig. 5. Plate VIII.] but elliptical, according to the ſection *e F g h i*. The sky thus ſeen ſtrikes the eye in the ſame manner as the long arch'd roof of the iſle of a cathedral church. or the cieling of a long room.

This being premis'd: Let us conſider the eye at C (Fig. 5.) upon the ſurface of the earth; and imagine C at the ſurface to coincide with K at the centre; to avoid taking into conſideration that the moon is really farther from the eye when in the horizon, than when it is ſome degrees high. Now when the moon is at G, we conſider it as at *g*, not much farther

farther than G: But when it is at H, we imagine it to be at *h*, almoſt as far again. While, therefore, it ſubtends the ſame angle nearly as it did before, we imagine it to be ſo much bigger, as the diſtance ſeems to us to be increas'd.

To illuſtrate this, the Dr. contriv'd the following experiment:

He took two candles A B, C D, (Fig. 6.) of equal height and bigneſs; and having placed A B at the diſtance of ſix or eight foot from the eye, he placed C D at double that diſtance; then cauſing any unprejudiced perſon to look at the candles, and asking which was the biggeſt. The ſpectator anſwer'd, they were both of a bigneſs; and that they appear'd ſo, becauſe he allow'd for the greater diſtance of C D; and this likewiſe appeared to him when he look'd through a ſmall hole. Then deſiring the ſpectator to ſhut his eyes for a little, the Dr. took away the candle C D, and placed the candle E F cloſe by the candle A B; and tho' it was as ſhort again as the others, and as ſmall again in diameter, the ſpectator, when he opened his eyes, thought he ſaw the ſame candles as before. Whence it is to be concluded, that when an object is thonght to be twice as far from the eye as before, we think it to be twice as big, though it ſubtends but the ſame angle.—— And this is the caſe of the moon, which appears to us as big again, when we ſuppoſe it as far again, though it ſubtends but the ſame angle.

The difference of diſtance of the moon in *perigæo* and *apogæo*, will account for the different bigneſs of the horizontal moon at different times, adding likewiſe the conſideration of the faintneſs, which vapours ſometimes throw on the appearance.

An Explication of the preceding Experiment to account for the appearance of the horizontal Moon; by the ſame. Phil. Tranſ. N° 444. p. 392.

DR. *Deſaguliers* made an experiment with three ivory balls for confirmation of what he had advanced; namely, that the deception ariſes from our judging the horizontal moon to be much farther than it is, which is as follows:

1. Two equal ivory balls (Fig. 7. Plate VIII.) were ſet one beyond the other in reſpect of the eye at E; namely A B at 20 foot diſtance from the eye, and C D at 40.

2. It

2. It is certain by the rules of optics, that the eye at E or F ſhall ſee the ball C D under an angle, but half as big as it ſees the ball A B; that is, that the ball C D muſt appear no bigger than the ball *o p* placed by the ſide of A B.

3. But when looking at the two balls with the naked eye in an open room, we conſider that C D is as far again from the eye as A B, we judge it to be as big as A B, (as it really is) notwithſtanding it ſubtends an angle but of half the bigneſs.

4. Now if unknown to the ſpectator, or while he turns his back, the ball C D be taken away, and another ball *o p* of half the diameter be placed in the ſame line, but as near again, at the ſide of A B, the ſpectator thinking this laſt ball to be at the place of C D, muſt judge it to be as big as C D; becauſe it ſubtends the very ſame angle as C D did before.

It therefore follows, that if a ball be imagined to be as far again as it really is, we make ſuch an allowance for that imagined diſtance, that we judge it to be as big again as it is, notwithſtanding that the angle, under which we ſee it, is no greater than when we look at it knowing its real diſtance.

For this reaſon the moon looks bigger in the horizon, and near it, than at a conſiderable height, or at the zenith; becauſe it being a common prejudice to imagine that part of the ſky much nearer to us which is at the zenith, than that part towards the horizon: When we ſee the moon at the horizon, we ſuppoſe it much farther; therefore, as it ſubtends the ſame angle, or nearly the ſame, as when at the zenith, we imagine it ſo much bigger, as we ſuppoſe its diſtance greater.

The reaſon why this experiment is hard to make is, becauſe the light from the ball *o p* is too ſtrongly reflected on account of its nearneſs: But if we could give it ſo little light as to look no brighter than the ball C D it would deceive every body. The Dr. made the experiment ſo as to deceive ſuch as were not very long-ſighted: But he owns he found it very hard to deceive thoſe who ſee at a great diſtance; tho' they would all be deceiv'd, if the diſtances were of 300 or 600 foot. Now in the caſe of the moon, the deception is help'd; becauſe the vapours, through which we ſee it when low, take away of its brightneſs; and therefore have the

ſame effect as would, or does happen in the experiment, when the light of the ball *o p* ſtrikes the eye no ſtronger than the light of the ball C D.

Some Obſervations on a Man and Woman bit by Vipers; by Dr. Atwell. Phil. Tranſ. N° 444. p. 394.

July 3. 1734. THE ſame man, who had been bit formerly by a viper in the preſence of ſeveral members of the *Royal Society*, was again bit in the preſence of ſeveral others beſides Dr. *Atwell*, in the publick hall of *Exeter College* at *Oxford*. He receiv'd two punctures in the wriſt, a little above the thumb; the blood iſſued, and more venom lay upon the orifices than could be immediately imbibed. He complained in about half an hour's time, that the poiſon was got up to his ſhoulder, and was entering his body. But notwithſtanding this, he was not ſuffered to apply his remedy (*ſallad oil*) till one hour and 10 minutes after he was bitten; by which time he began to be floſhed and in a ſweat, his hand ſwoln and diſcoloured.

Upon an application of his medicine, he found ſome abatement of his pain; but the ſwelling appear'd more viſible, and ſpread itſelf farther into his arm. In about a quarter of an hour the man ſunk under the table, and complain'd of violent pain in his back and bowels, nor could he bear to be mov'd. At laſt, his pulſe failing, his jaw being fallen, his countenance changed, and eyes fixt, he was ſtretch'd upon the table, and the medicine applied to his belly, and ſtomach. Soon after which, recovering a little, he began to vomit, and he brought up more than a quart of phlegm and bile. In this condition he lay for more than an hour, and then was remov'd into the Doctor's lodgings; where he was ſeiz'd again with a fit of vomiting, and likewiſe purging, and continued ſo till midnight. The Doctor kept him in his houſe upwards of an hour in hopes of his growing better; but his diſorder ſtill continuing, and he being too weak and feeble even to ſtand, the Doctor ſent him home in a chair to his own lodgings, where he was put into bed, and after midnight fell aſleep, and waked next morning perfectly well, only that his arm was ſtill ſwoln, and the fleſh pitted, as if it had been dropſical. His arm was bound up in papers dipt in his own medicine (*ſallad-oil*) and this was all, as far as the Doctor could obſerve or learn, that was applied to it.

The

The ſame day two young chickens were bit; one died in two hours, and the other in four hours time. A third was bit three times; and then had the medicine applied; but it died at the end of 10 hours. The fleſh of this laſt was grown very black, and there was much extravaſated lymph between it and the skin, which ſtunk intolerably: But the Doctor could not perceive, that the viſcera were at all diſcolour'd.

July 4. Another fowl, half grown, was bit in two places, and the medicine was applied; in half an hour after, the fowl eat and ſeem'd much recover'd, but was dead in 14 hours time.

July 6. Two half grown cocks were bit; the firſt was bit but once, yet violently, and turned black immediately. It had the medicine applied, eat afterwards, and ſeem'd pretty well; yet died in 20 hours. The other was bit two or three times, but hardly wounded, and not half ſo much diſcolour'd as the former: The wound was bath'd with viper-oil, but the fowl died in a little more than two hours.

July 8. Two young pigeons were bit, the one had viper-oil applied immediately, but it ſicken'd and died in four hours: The other had oil-olive applied, and recover'd perfectly: the fleſh beginning to return to its natural colour in about an hour's time.

July 17. The woman was bit in the publick hall of *Brazennoſe-College*, in the preſence of Dr. *Frampton*, Dr. *Frewen*, and ſeveral other phyſicians. It had been ſuſpected, that they play'd ſome tricks with their vipers, and made them ſpend their rage and venom beforehand: To obviate which a phyſician of the company had provided ſome freſh vipers, which he had caught himſelf a day or two before, and kept in his own cuſtody till that time. The woman was bit twice by one of theſe, and receiv'd three wounds; one in the thumb, and two in the fore-finger: Her hand was ſoon ſwoln and ſpotted, and her finger turn'd black. After 23 minutes ſhe applied the medicine to her hand, but no farther than the ſwelling went, by which Dr. *Atwell* ſuſpects the following illneſs was in ſome meaſure occaſion'd. She walk'd home very well in appearance. But in about three hours after the bite was receiv'd, ſhe grew very ſick and was in great pain; and was ſeiz'd with vomiting, purging, fainting fits, which continued upon her all night; inſomuch that the people of the houſe deſpair'd of her life: Nor had ſhe any ſleep

till noon the day following. The Dr. saw her about 6 that evening, when she awaked, he found her very well in spirits, but complaining of most acute pains in her finger. Her arm, shoulder, back, and breast, on that side, were much swoln and inflamed: All those parts thus affected were bound up in papers soak'd in the medicine. After this there appear'd upon her finger 2 large bladders, full of a black corrupt matter; and this not only upon the wound, but one of them was upon a part of the finger distant from it. She could not be persuaded to open them, which the Dr. believes would have eas'd her very much.

July 20. the swelling was considerably abated, and almost reduced entirely into her hand, which began to pit: But she complained still of her finger; and could hardly endure to have it dress'd with fresh papers. She continu'd in bed till the 22d, for the sake of keeping her hand in a more easy posture; and then she came abroad.

The same day that the woman was bit, they caus'd a fowl to be bit; but the wound was not deep, and little more than a scratch. Nothing was applied to it, and it died in 24 hours.

A large puppy was bit the same day 3 times in the head, had the medicine applied, but it died in about an hour.

As it was known that the man and woman kept themselves fasting on those days when the experiment was to be tried upon them: This occasion'd a suspicion that they might take some antidote to prepare their bodies; for which reason Dr. *Atwell* order'd the man to bring him some vipers after dinner, under pretence of making some farther experiments on dogs; providing at the same time some fresh vipers without his knowledge, and then the Dr. propos'd to him to be bit by one of them, and apply his medicine immediately. His hand was besmear'd with the medicine in applying it to a young dog, on which the experiment was just made. Two vipers were tried on the man, but neither would bite him: One of them attempted it several times, and spilt his venom, but always caught back his head again, as if there had been something offensive in the hand. Upon this, suspecting that the smell of the medicine might occasion it, he was made wash his hand, after which another viper bit him immediately: But whether this conjecture was right or not must depend upon farther trial. The man receiv'd the bite on the joint of the thumb, and the blood issu'd at the 2 orifices. He applied the medicine directly: The thumb soon appeared black, the hand was swoln, and the flesh pitted immediately

mediately. He drank a mug of ale after it, and then went home to bed. His whole arm was ſwoln; but he was ſo well that he went 6 miles out of town, and came home again in the evening. The Dr. ſaw him again, when the ſwelling was almoſt gone above the elbow, but the fleſh pitted below: The wound had bliſter'd, but the bladders were fill'd with a water, and not any thing of that black matter, which appear'd on the woman's finger.

A young dog, mention'd before, was caus'd to be bit the ſame day, and the medicine applied: Another dog was bit 3 times in the noſe, and nothing applied: Both were much ſwoln, but very likely to live.

The teeth of a viper's head cut off 24 hours before, was likewiſe thruſt into the fleſh of a fowl, which turned black immediately, but the fowl perfectly recover'd without any application.

Some electrical Experiments intended to be communicated to the Royal Society; *by Mr.* Stephen Gray, *and taken from his Mouth the Day before he died; by Dr.* Mortimer. Phil. Tranſ. N° 444. p. 400.

Exp. 1. TAKE a ſmall iron globe of an inch or an inch and a half in diameter, which ſet on the middle of a cake of roſin of about 7 or 8 inches diameter, having firſt excited the cake by gently rubbing it, clapping it 3 or 4 times with the hands, or warming it a little before the fire: Then faſten a light body, as a ſmall piece of cork, or pith of alder to an exceeding fine thread, 5 or 6 inches long, which hold between your finger and thumb, exactly over the globe, at ſuch a height, that the cork, or other light body, may hang down about the middle of the globe: This light body will of itſelf begin to move round the iron globe, and that conſtantly from weſt to eaſt, being the ſame direction which the planets have in their orbits round the ſun. If the cake of roſin be circular, and the iron globe placed exactly in the centre of it, then the light body will deſcribe an orbit round the iron globe, which will be a circle: But if the iron globe be placed at any diſtance from the center of the circular cake, then the light body will deſcribe an elliptical orbit, which will have the ſame eccentricity, as the diſtance of the globe from the centre of the cake.

If the cake of roſin be of an elliptic form, and the iron globe be placed in the centre thereof, the light body will deſcribe

ſcribe an elliptical orbit, of the ſame eccentricity as the form of the cake.

If the iron globe be placed in or near one of the *focus*'s of the elliptic cake, the light body will move much ſwifter in the apogean than in the perigean part of the orbit, contrary to what is obſerv'd of the planets.

Exp. 2. Take the ſame or ſuch another iron globe, and having faſten'd it on an iron pedeſtal about an inch high, ſet it on a table; then ſet round it a glaſs hoop, or portion of a hollow glaſs cylinder of 7 or 8 inches diameter, and 2 or 3 inches high: This hoop muſt be firſt excited by warming and gently rubbing it; then hold the light body ſuſpended as in the firſt experiment, and it will of itſelf move round the iron globe from weſt to eaſt in a circular orbit, if the hoop be circular and the globe ſtand over the centre thereof; but in an elliptic orbit, with the ſame eccentricity, if the globe does not ſtand in the centre of the hoop, as in the firſt experiment, when the globe does not ſtand on the centre of the cake.

Exp. 3. This ſame iron globe being ſet on the bare table, without either the cake of roſin or glaſs hoop, the ſmall light body being ſuſpended as in *Exp.* 1, 2, will make revolutions round it, but ſlower and nearer to it than when it is placed upon a cake of roſin, or within a glaſs-hoop.

Mr. *Gray* had not found that theſe experiments would ſucceed, if the thread, by which the light body was ſuſpended, was ſupported by any other thing than a human hand: But he imagin'd it might happen the ſame, if the thread ſhould be ſupported or faſten'd to any animal ſubſtance whatever; and he intended to have tried the foot of a chicken, a piece of raw fleſh, or the like.

Mr. *Gray* thought to explain the foregoing particular by the following odd phenomenon, of which he aſſur'd Dr. *Mortimer* he was very certain, having often obſerved it; *viz.* if a man reſting his elbows on his knees, places his hands at ſome ſmall diſtance from each other, they will gradually accede to each other, without any will or intention of the man to bring them together; and they will again recede of themſelves. In like manner the hand will be attracted by the body; or the face of a man, if he ſtand near a wall, will be attracted to the wall, and be again repell'd by it.

Mr. *Gray* told the Dr. he had thought of theſe experiments only a very ſhort time before his falling ſick, that he had not

tried

tried them with variety of bodies, but that from what he had already ſeen of them, which ſtruck him with new ſurprize every time he repeated them, he hoped, if God would ſpare his life but a little longer, he ſhould, from what theſe phenomena point out, bring his electrical experiments to the greateſt perfection; and he did not doubt but in a ſhort time he would be able to aſtoniſh the world with a new ſort of *planetarium* never before thought of; and that from theſe experiments mihgt be eſtabliſhed a certain theory for accounting for the motions of the grand *planetarium* of the univerſe.

In trying theſe experiments ſince Mr. *Gray*'s death, the Dr. found that the ſmall light body will make revolutions round a body of various ſhapes and ſubſtances, as well as round the iron globe, if ſet on the cake of roſin. Thus he tried with a globe of black marble a ſilver ſand-diſh, a ſmall chip box and a large cork. He obſerv'd that the cake, if nothing ſtood upon it, would in any part ſtrongly attract the light body, as held ſuſpended by the thread; but when the globe or other body was ſet upon it, the edges of the cake attracted the ſtrongeſt; and ſo gradually the attraction ſeem'd as it approach'd the centre to become leſs, till at a certain diſtance it was changed into repulſion, which proceeded from the globe, or other body placed upon the cake, which very ſtrongly repels the light body, unleſs it be held very near it, and then it attracts it ſtrongly. While the light body is ſuſpended, as in the foregoing experiments, if you approach the finger of the other hand near it, it will fly from the finger, or be very vigorouſly repell'd by it.

Some thoughts on the Sun *and* Moon *appearing bigger when near the Horizon than when near the Zenith; by Mr.* James Logan. Phil. Tranſ. N° 444. p. 404.

IT may, perhaps, be needleſs to add any thing in confirmation of Dr. *Wallis*'s ſolution (vide *Phil. Tranſ.* N° 187.) of the ſun and moon's appearing ſo much larger at riſing or ſetting than when in a greater altitude; tho' ſome have very abſurdly ſtill gone on to account for it from vapours. It is true, indeed, that it is theſe vapours, or the atmoſphere alone, that make thoſe bodies, when very near the horizon, appear in a ſpheroidal form, by refracting, and thereby raiſing (to ſight) the lower limb more than the upper; yet theſe can be no cauſe of the other phenomenon. The ſun and moon, each ſubtending about half a degree, appear in the meridian of the breadth of 8 or

10

10 inches, to ſome eyes more, and to others leſs; and in the horizon to be 2 or 3 foot, more or leſs, according to the extent of ground they are ſeen over. But if one can have an opportunity as Mr. *Logan* frequently had at *Philadelphia*, of ſeeing the ſun riſe or ſet over a ſmall eminence at the diſtance of a mile or two with tall trees on it, ſtanding pretty cloſe, as is uſual in woods without underwood, his body will then appear to be 10 or 12 foot in breadth, according to the diſtance and circumſtances of the trees he is ſeen thro': And where there has been ſome thin underwood, or a few ſaplings, Mr. *Logan* has obſerv'd that the ſun ſetting red, has appear'd thro' them like a large extenſive flame, as if ſome houſe were on fire beyond them. Now the reaſon of this is obvious, *viz.* that being well acquainted with trees, the idea's of the ſpace they take up are in a manner fix'd; and as one of thoſe trees ſubtends an angle at the eye, perhaps not exceeding 2 or 3 ſeconds, and would ſcarce be diſtinguiſhable, were it not for the ſtrong light behind them, the ſun's diameter of above 30 minutes takes in ſeveral of them; and therefore will naturally be judged vaſtly larger. Hence it is evident, that thoſe bodies appear greater or leſs, according to the objects that interpoſe or are taken in by the eye on viewing them. And to this only is that phenomenon to be imputed.

Mr. *Logan* is ſenſible this method of arguing is not new; yet the obſervations here given may probably tend to illuſtrate the caſe beyond what has been advanced on the ſubject.

The Caſe of a Lad *bitten by a mad* Dog; *by Mr.* Edward Nourſe. Phil. Tranſ. N° 445. p. 5.

STEPHEN *Bellaſs*, about 16 years of age, was ſome time in *June* 1735 bit by a mad dog thro' the nail of his right thumb. Mr. *Nourſe* being immediately call'd, propos'd to make a ligature above, and cauterize the wounded part: But that not being complied with, he deſir'd Mr. *Gernum*, the apothecary, to make up the remedy, mention'd by *Dampier* in *Phil. Tranſ.* N° 237 and 443, viz. ℞. *Lichen. ciner. terreſtris, piper nig. a a* ℥ 1. *f. pulvis.* Of this powder the lad took a drachm within an hour after he was bit; repeated it next morning before he ſet out for *Graveſend*, where he was 10 days, and dipt in the ſalt water every day; during which time he repeated the medicine evening and morning; and he continu'd ſo to do for 40 days.

He

He had not the leaſt ſign of being affected by the poiſon, till *Tueſday* the 11th of *January* 1736-7, when in the evening he complain'd of a numbneſs in 3 of the fingers of the hand that was not bit: Next morning he was ſick, had great pain acroſs his ſtomach and in all his bones; in the evening Mr. *Nourſe* was ſent for to bleed him, the people about him ſuppoſing he had got cold. When he came he found him feveriſh, with a hard full pulſe, and the complaints aforeſaid: Upon enquiring what nouriſhment he had taken that day? He made anſwer, none: For, he could not ſwallow: Whereupon Mr. *Nourſe* look'd into his mouth, but there was no inflammation; neither did any thing occur to him that could poſſibly produce the difficulty of ſwallowing, complain'd of: He was offer'd ſome ſack-whey in a baſon, but he ſtarted at the ſight of it, neither would he ſuffer it to come near him: He was then offer'd a ſpoonful, which he was prevail'd on to ſwallow; the moment it was down, he was convulſed, and a remarkable horror appear'd in his countenance, ſucceeded by a profuſe ſweat all over his face and head: He afterwards took another ſpoonful; the conſequence was the ſame as before, but in ſomewhat a higher degree: Mr. *Nourſe* was now convinced that this was the ὑδροφοβία, and that it aroſe from his having been bit 19 months before: For, after the moſt ſtrict enquiry, it did not appear that he had been bit by any animal ſince.

Mr. *Nourſe* acquainted his friends with his apprehenſions, and deſir'd farther advice. Upon which Dr. *Monro* was ſent for, who order'd him to be let blood, and to repeat the above-mention'd medicine in a bolus every 4 hours, and a clyſter: He was blooded, and the clyſter injected; but he could not be prevail'd on to take but one of the bolus's. This night was ſpent with great inquietude, and without any ſleep: *Thurſday* morning he was generally convulſed, and had frequent reachings and yawnings alternately: About noon his mind (which till then continued ſound) left him, and he rav'd and foamed at the mouth till 5 o'clock in the afternoon; at which time nature ſeemed quite ſpent, and he lay very quiet till 7, when he died.

Thus the poiſon lay latent in this lad near 19 months; a thing which never fell within Mr. *Nourſe*'s obſervation before, tho' mentioned in books.

Mr. *Nourſe* had cut this lad for the ſtone the preceeding ſummer; namely about a year after he had been bit; and he never ſaw a wound more diſpos'd to heal; for, the lad was well and abroad in 5 weeks.

An Explanation of the Runic Characters *of* Helsingland *by M.* Andrew Celsius. Phil. Trans. N° 445. p. 7.

IT is well known that there are stones found in several parts of *Sweden*, which were formerly set up as obelisks in memory of the dead. These monuments are marked with the ancient northern letters, call'd *Runor* or *Runic characters*. But in *Helsingland* a province of north *Sweden* there occur 5 of those stones, which have characters cut into them, that seem to differ from the common *Runic*. Upon the introduction of our modern letters, these *Runic* characters became so little regarded, that their interpretation was lost even to the *Swedish* antiquaries till the year 1674, when M. *Magnus Celsius*, Grandfather to our author, astronomy professor at *Upsal*, reviv'd their reading,, and drew up the alphabet of them (Fig. 1. Plate IX.) ranged after the manner of the ancients.

There are but 16 letters; and the words are frequently distinguish'd either by 3 points set perpendicularly over one another: or by two at some distance asunder.

Among the several alphabets hitherto known, it would be a hard matter to find one like the foregoing, except the characters of the *Persepolis* inscriptions, which have not hitherto been decypher'd: For, the letters generally made use of denote different sounds, according to their various shapes: Whereas in this alphabet the same character often denotes a different sound, according to the diversity of its place and attitude between the two parallels. Thus a strait stroke, standing perpendicular to the parallel lines, signifies I, F, D and S: For, when it joins these parallels, it signifies I; when it rests on the lower parallel, it signifies F, on the upper, S; and D, when it touches neither of them. The small wedge leaning to the right, and placed near the upper parallel, denotes L; in the middle, N; and O, near the lower parallel; a line descending from the upper parallel, and making a curve downwards to the left, stands for K; the same placed contrarywise from the lower parallel upwards, expresses R; and so of the rest.

The intention of the first inventor of these letters seems to have been, to form all the characters of small wedges, straight and crooked lines, and 2 points, variously placed between

ween the 2 parallels: For, the wedges may be placed 15 different ways, as represented in Fig. 2.

The straight line may also have 15 different situations, as in Fig. 3.

The crooked lines may likewise be varied 14 different ways, as in Fig. 4.

In fine the 2 points admit of 12 variations, as in Fig. 5.

But as the ancient *Suevo-Gothi* had but 16 letters in their alphabet, they did not want all these variations of the wedges, lines and points: Wherefore they employ'd six variations of the wedges; five of the straight lines; three of the crooked; and but two of the points.

If we now suppose these *Helsingic* characters to be older than the common *Runics*; the greatest part of the common *Runics* may easily be deriv'd from the *Helsingics*, by adding a perpendicular line to the small wedges and curves; as appears by Fig. 6.

But if we suppose the common *Runics* to be older, and to be deriv'd, as is very probable, from the ancient *Greek* and *Roman* letters; we must contrarywise deduce the *Helsingic* characters from the common *Runics*, by substracting the perpendicular line.

As a specimen Fig. 7. represents a stone found at *Malstad*: M. *Celsius* took an exact copy of it *Anno* 1725, in company with his uncle Dr. *Olave Celsius*; of whom is expected a complete account of all these *Helsingic* inscriptions.

On the outer limb or border is what is represented Fig. 8.

In the first curvature as in Fig. 9.

In the second snake dragon as in Fig. 10.

In the inner limb as in Fig. 11.

In the 2d curvature as in Fig. 12.

In the first snake as in Fig. 13.

In the heads of the snakes as in Fig. 14.

That is, *Frumunt* erected this stone to *Fisiulfi* the son of *Brisi*: But *Brisi* was the son of *Lini*: But *Lini* was the son of *Un*: But *Un* was son of *Fah*: But *Fah* the son of *Duri*: But he the son of *Barlaf*: But he (the son) of *Drun*: But he (the son) of *Lanas*: But he (the son) of *Fidrasiv*. *Frumunt* the son of *Fisiulfi* made these *Runic* (letters). We have placed this stone to the north of *Bala* stone. *Arva* was the mother of *Fisiulfi*. *Siulfir* (or *Fisiulfir*) was the Governour of this province. His place of abode was in *Rimbiun*.

That this monument was erected since Christianity began to flourish in *Sweden*, sufficiently appears by the figure of the cross. It is moreover probable, that *Fisiulfi*, as the Governour of the province, was descended of a very noble family; seeing his genealogy is traced 10 generations backwards: Now if we suppose *Frumunt* to have been 30 years of age when he erected this monument for his father, and with Sir *Isaac Newton* allow 30 years for each generation; we shall find 330 from the death of *Fisiulfi* to the birth of *Fidrasiv*, who is the stock of these generations.

There is a figure of this stone in M. *De la Motraye*'s travels; but with considerable errors in the windings of the snakes, and in the letters, as well as in the explanation given them.

A Lunar Eclipse *observ'd in* Fleet-street, London, March 15, 1735-6; *by Mr.* George Graham. Phil. Trans. N° 445. p. 14.

H.	M.	S.	
10	13	0	The beginning.
11	11	0	The total immersion.
12	49	0	The emersion.
13	47	0	The end.

At *Greenwich*, Dr. *Halley* observ'd.

	H.	M.	S.
The beginning	10	13	37
The immersion	11	9	42.

Observations on the lunar Eclipse March 15, 1735-6 *at Mr.* Graham's *House in* Fleet-street, London; *by M.* Celsius. Phil. Trans. N° 445. p. 15.

THIS eclipse was observ'd with a reflecting telescope, of 11 inches, magnifying 63 times and made at *Edinburgh*.

H.	M.	S.	
10	22	5	The shadow on the middle of *Kepler*.
	23	15	Entering *Mare humorum*.
	28	16	—— *Copernicus*.
	29	34	—— the middle of *Copernicus*.
	30	26	*Copernicus* entire.

H.

PLATE. IX

Fig. XVI.

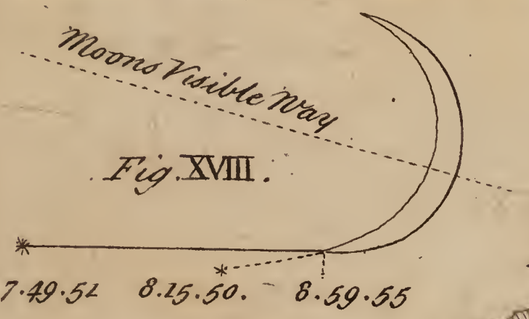


Fig. XVIII.

7.49.51 8.15.50. 8.59.55

Fig. VII.

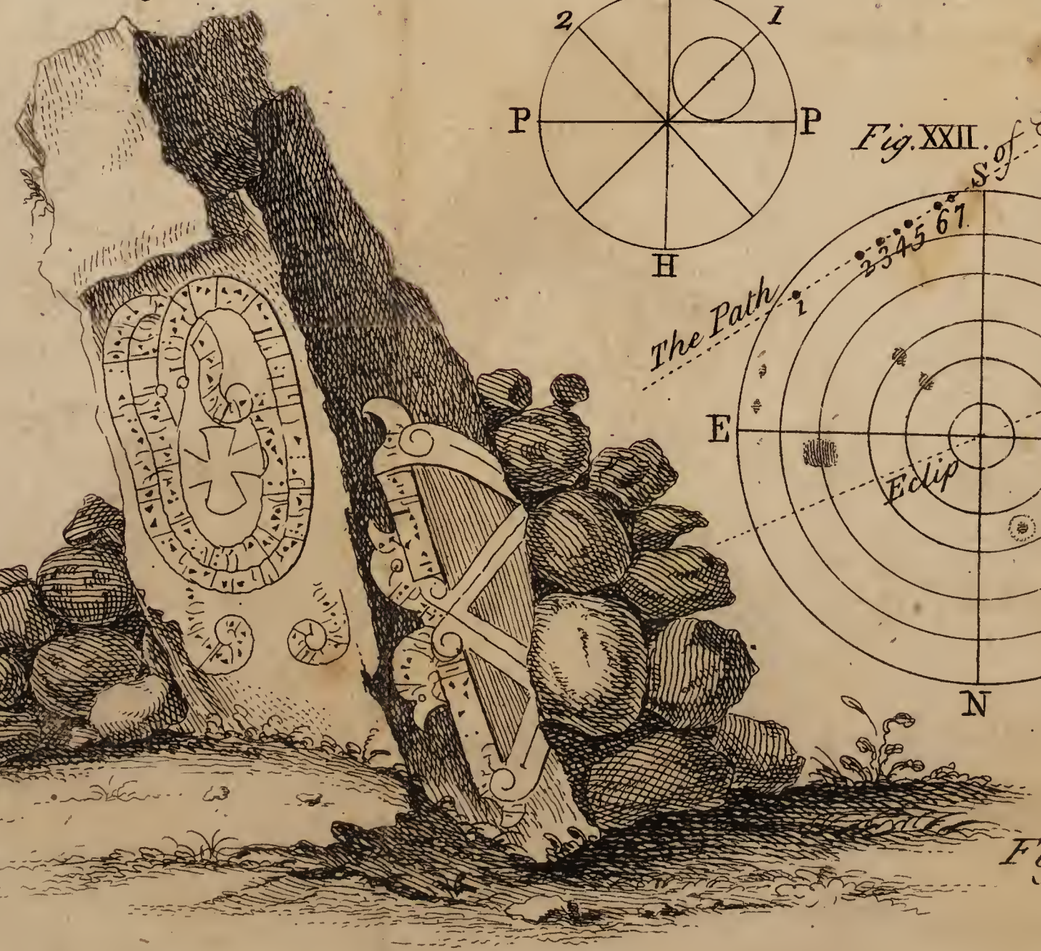


Fig. XI.

ST IN DINA NURI, BALASTIN

Fig. XII.

AR VA VAS MUDIR FISIULFI

Fig. XIII.

SIULFIR VARD UMLAN TIDISURI VISR.

Fig. XIV.

SIN VANU IN RIMBIUM.

Fig. XVII.

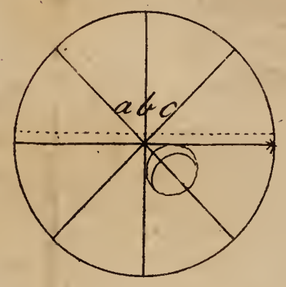


Fig. XV.

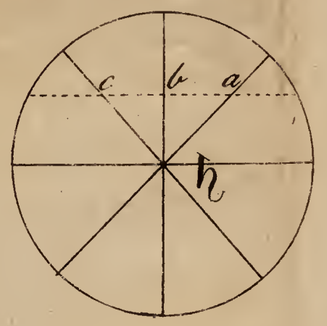


Fig. XIX.

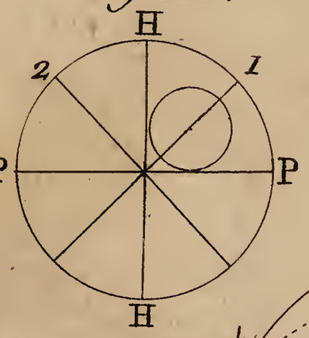


Fig. XX.

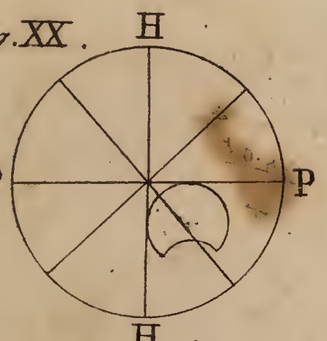


Fig. XXI.

Fig. XXII.

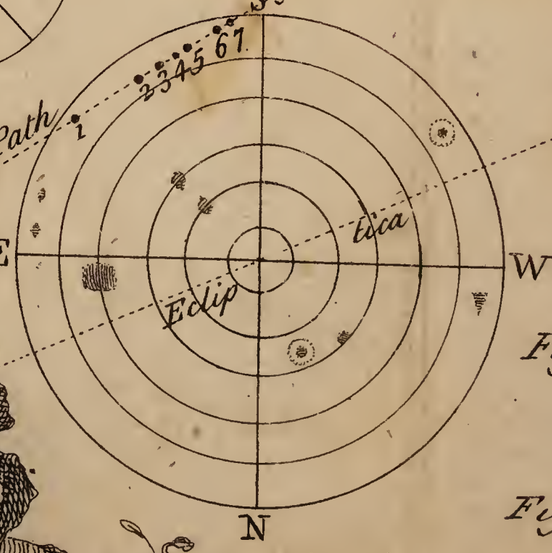


Fig. I.

F VDORK HNIAST

Fig. II.

1 2 3 4 5 6 7 8 9 10 11 12

Fig. III.

1 2 3 4 5 6 7 8 9 10 11 12

Fig. IV.

1 2 3 4 5 6 7 8 9 10 11 12 13

Fig. V.

1 2 3 4 5 6 7 8 9 10

Fig. VI.

V O R K N I A

Fig. VIII.

FRUMUNT FST STAINA DI

FISIULFA, BRISASUN; IN BRISI

SUN; IN LINI VAS UNAR SUN;

FAH SUN; IN FAHA DURI SUN.

Fig. IX.

IN DA BARLAF; IN DA SUDR

Fig. X

IN DA LANASR; IN DA FIDR

Fig. XI.

FRUMUNTFISIULFA SUN FADIR UNARDISAR

G. Smith del.

H.	M.	S.	
10	33	28	ſhadow enters on *Timocaris*
	38	44	— On *Tycho*
	39	12	— On the middle of *Tycho*
	40	48	*Tycho* entire
	46	0	The ſhadow enters on *Menelaus*
	49	20	On *Plinius*
11	0	40	On *Mare Criſium*
	5	36	*Mare Criſium* entire
	9	17	The total immerſion is about to begin
13	13	55	*Tycho* emerged out of the ſhadow
	29	0	*Mare ſerenitatis* entirely emerged
	40	45	*Mare Criſium* entirely emerged
	45	50	The eclipſe nearly ended
	46	12	Certainly ended.

Obſervations on the ſame Eclipſe, in Covent-Garden, London; *by Dr.* Bevis. Phil. Tranſ. N° 445. p. 16. *Tranſlated from the* Latin

True time p. m.

H.	M.	S.	
6	53	47	*Saturn* in the point of interſection of the threads of the micrometer.
7	31	5	The firſt of the hyades at ♌ paſſes the thread *a* (Fig. 15. Plate IX.) the teleſcope inverting.
7	31	50	— Paſſes the horary thread *b*.
7	32	35	— Paſſes the thread *c*.
7	42	39	*Saturn* again in the concourſe of the threads.
8	19	57 ½	The firſt of the hyades at ♌ paſſes the thread *a*
8	20	42 ½	— *b*
8	21	27 ½	— *c*
9	50	0	The diſk of the moon runs along the horary thread in 139 ſeconds of time
9	56	0	Again in 139 ſeconds
10	01	0	And again in 139 ſeconds
10	9	40	A thin penumbra ſeems to darken the moon near *Hevelius*
10	10	20	Now a very ſenſible one
10	11	40	The beginning

True

True time p. m. H.	M.	S.	
10	14	38	The limits of the ſhadow, as far as he could judge, paſſes through *Grimaldus* and *Cavalerius*
10	19	46	Through *Ariſtarchus*
10	24	15	The ſhadow enters on *Mare humorum*
10	32	44	It covers *ſinus roris*
10	32	44	The moon ſcarcely ſeen through the clouds
10	40	18	The ſhadow divides *Tycho*
10	42	26	It touches *Mare ſerenitatis*
10	46	1	It touches *Menelaus*
			A black cloud comes on.
10	53	46	The cloud going off, *Mare nectaris* is found entirely immerged.
			Very thick clouds do again intercept the ſight of the moon
11	0	56	The ſhadow touches *Mare Criſium*
11	5	48	*Mare Criſium* and *mare fœcundum* immerged
11	10	0	The moon entirely immerged.
12	42	20	Now the eaſtern limb of the moon begins to clear up
12	46	5	It grows ſtill clearer
12	46	56	A thread of pure light reſtor'd in the twinkling of an eye
			A great many flying ſmall clouds
12	57	5	The limits of the light touch *mare humorum*
13	4	3	*Mare humorum* intirely emerged
13	13	40	*Tycho* half emerged
13	14	0	Entirely emerged
13	17	22	*Waltherus* emerges

A great darkneſs by clouds, that, as it ſeem'd, would continue for ſome time

13	43	44	*Mare fœcundum* is obſerv'd without the ſhadow
13	46	25	The true ſhadow ends
13	48	30	The penumbra no longer ſenſible

In theſe obſervations the Doctor made uſe of a good clock that for ſeveral days before, and on the very day of the eclipſe had been adjuſted by five correſponding altitudes of

of the ſun, corrected by the equation table, as alſo of a ſix foot teleſcope. About the middle of the eclipſe the moon was ſeen as through a duſkiſh ſmall cloud; but at the edges ſhe was red as glowing hot iron. The limits of light and ſhadow were all the time of the eclipſe not well defin'd.

Obſervations of the ſame Eclipſe, at Yeovil in Somerſetſhire, *Lat.* 50 *Degrees*, 52 *Minutes*; *by Mr.* John Milner. Phil. Tranſ. N° 445. p. 18.

The clock was firſt adjuſted by the equation table.

				Deg.	Min.
The beginning of the eclipſe at	10	6	0		
The moon's altitude then was				34	29
The beginning of total obſcuration	11	4	30		
The moon's altitude then was				34	16
The middle of the total obſcuration	11	54			
The end	12	43	30		
The moon's altitude then was				34	47
The end	1	39	15		
The altitude then was				31	47
The continuation of the total obſcuration was		1	39		
The duration of the whole eclipſe	3	33	15		

The Efficacy of Oil-olive *in curing the Bite of Vipers*; *by Mr.* Stephen Williams. Phil. Tranſ. N° 445. p. 26.

AT *Plymouth*, *June* 26, 1735, in preſence of ſeveral gentlemen of the faculty of phyſic, *William Oliver*, the viper-catcher (mentioned *Phil. Tranſ.* N° 443) ſuffer'd himſelf to be bit by a female viper; which being enraged, fix'd her fangs in the middle part of his fore-finger. There ſoon iſſued out blood from the wounds: But that the poiſon might the more ſtrongly appear, the ſame viper immediately bit a pigeon in the breaſt, which expired in leſs than half an hour. Another pigeon was alſo bit by the ſame viper, which likewiſe expired, tho' not ſo ſoon as the firſt. The man immediately complain'd of an acute pain in the wounded part; and it ſoon look'd red, and then became of a livid colour; his finger ſwelled to a great ſize; and he could not bend it. Soon after this his hand alſo began to ſwell: He complained of faintneſs, and pains flying to his arm, ſhoulder,

der, and arm-pit. In half an hour's time from the bite, his specific being applied, and ſtrongly rubbed into the part affected, procured him immediate eaſe: His pain abated, his finger became flexible, and his ſpirits ſeem'd more chearful: The ſpecific being ſeveral times repeated and applied, his pains gradually diminiſh'd. The next day his finger and hand continued ſwoln, but without pain; the skin began to appear yellow, and puſtules like bladders appear'd on his finger; which being prick'd emitted a ſanious liquor. In two days time all the ſymptoms vaniſhed, and he became perfectly well.

June 30. The gentlemen of the faculty met again, when ſeveral experiments were tried upon puppies, cats, and pigeons; wherein was found the efficacy of this man's ſpecific, and that to the great ſatisfaction of the company.

A Propoſal for the Admeaſurement of the Earth in Ruſſia; *by Mr.* Joſ. Nich. de L'Iſle. Phil. Tranſ. N° 445. p. 27.

NEceſſity, or the exigencies of geography and navigation, put mankind very early upon the enterpriſe of meaſuring the earth: For, how is it poſſible to conſtruct maps of each kingdom or empire, without ſetting down all the places in their true diſtances, by the meaſures made uſe of in each country; as were the *ſtadia* of the ancients; and now the miles, leagues, werſts, *&c.* of the moderns. And how could different ſtates be compar'd with one another; ſo as to come at the knowledge of the ſpaces they ſeverally poſſeſs on the earth's ſurface, without knowing the number of theſe common meaſures, contain'd in a degree, or in the whole extent of the earth? Hence aroſe the twofold method of determining the ſituation of the different parts of the earth, either by their mutual diſtances, ſet down in the meaſures made uſe of in each country; or expreſſed in meaſures common to all, as degrees, minutes, and ſeconds, by marking the longitude and latitude of each place.

Upon the firſt determination of the magnitude of the earth in geographical meaſures, as in *Stadia* and *Arabian* miles, the ancients did not employ any great degree of exactneſs: They were content to ſet down the circumference of the earth and of its parts, in round numbers; probably, becauſe they did not expect to be able to attain to much preciſeneſs in a reſearch of this nature: But according as their deſires of improving geography increaſed, by entering into a detail of it, they found it neceſſary to have a more exact knowledge of the magnitude

of

f each degree, not only in the great measures, as miles and eagues, but also in perches, toises and feet; which could not e otherwise done than by geometrical operations and stronomical observations, more exact, and consequently more perose, than had been, or indeed could have been, under-aken before.

M. *de L'Isle* avoids entering upon a detail of the immense abours of modern Mathematicians on this head; as those f *Fernel* in *France*; *Snellius*, *Bleau* and *Muschenbroek* in *Holland*; *Norwood* in *England*; *F. Riccioli*, and *M. Bianchini* n *Italy*; and the gentlemen of the *Academy of Sciences* in *France*; to get only the precise magnitude of a degree in he measures of their respective countries: But he answers an bjection, which might be rais'd hereon, *viz.* that it was needless to undertake these same operations in so many dif-erent places; since the magnitude of a degree once deter-min'd in the measures of any one country, may be easily re-duced to the measures of any other, by the exact knowledge we now have of the proportions of modern measures.

Now taking *Russia*, for instance, the geographical measures of which are werfts, divided each into 500 *sagenes*, and each *sagene* supposed to be exactly seven feet *English*, this rela-ion once known, as also the exact relation of the *English* to he *French* foot, or to the toise of six foot, which the *French* astronomers made use of in their admeasurements; and of which they found a degree of a great circle contained 57060; what more is requisite for concluding that a degree of a great circle contains 104 werfts and $\frac{1}{2}$? And what remains to the perfection of the geography of *Russia* in the most mi-nute detail that can be enter'd upon, but to employ this measure of werfts, sagenes, and *English* feet (if you please) in actual admeasurements; and to construct the maps by the most exact methods of geometry; taking care to set them down right, as to their true bearings, and to regulate them by the most exact astronomical observations of longitude and la-titude that can possibly be made.

But M. *de L'Isle* makes it appear, that the geography of *Russia* cannot be brought to this pitch; not only in the gene-ral map, but likewise in that of any particular district what-soever; without undertaking an equal, and even a greater work than all that hitherto has been done in *France* and else-where, towards the admeasurement of the earth.

When it was ſaid above, that an exact knowledge of the magnitude of a degree of the earth in any known meaſures of one country was ſufficient for conſtructing exact maps of all other countries, only having a regard to the different proportion of the meaſures, that is to be underſtood upon a ſuppoſition of the earth's being perfectly ſpherical: Seeing it is well known, that in a ſphere the degrees of all the great circles are every where equal; and that we likewiſe know, in a ſphere, the proportion of the degrees of the leſſer circles to their great parallels, according to their diſtance from them.

But if the earth be not perfectly ſpherical, the caſe is quite alter'd: All the degrees of the great circles will not be equal to one another, and thoſe of the leſſer, taken at certain diſtance from their parallel great circles, will not have the ſame relation that the degrees of the leſſer circles taken at the ſame diſtance, would have on a ſphere. In all this there might poſſibly ariſe an infinite variety, according to the figure the earth might have: And as it has not been hitherto decided, what is the true figure of the earth; and that there is no better method for aſcertaining it, than by obſervations made in ſo great an extent as that of *Ruſſia*.

For theſe reaſons M. *de L'Iſle* has advanced, that the perfection of the geography of *Ruſſia*, ſtands in need of this great undertaking: But before he enters into a detail of the great advantages of this reſearch, and the nature of the operations he propoſes, he thinks it neceſſary to explain in what manner he means that the queſtion of the earth's figure and magnitude is not yet decided,

There have been ſome who have long ſince ſuſpected, and even thought, they were furniſhed with proofs of the earth not being exactly ſpherical. M. *de L'Iſle* here intirely abſtracts from the unevenneſſes of its ſurface; which are no ſenſible in regard of the earth's whole bulk; ſeeing the top of the higheſt mountains, and thoſe even few in number are ſcarce more than a league above the level of the ſea. Wherefore, he ſuppoſes the earth to be bounded by a curve ſurface, ſuch as it would be by the level of the ſea carried quite over all the earth. It is in this manner, the earth being conſidered as cover'd with a fluid, that Sir *Iſaac Newton*, in the firſt edition of his *Principia*, publiſhed in 1686 has demonſtrated, that ſuppoſing this fluid homogeneous and the earth to have been at reſt at the time of its creation

ion, it must have assumed the figure of a perfect sphere: But afterwards supposing it to have a motion on its axis, as is well known it has in 24 hours; this spherical figure must have been changed into that of a spheriod, flatted at its poles, in which the degrees on the meridian must be greater, drawing nearer the poles than near the equator.

Sir *Isaac* confirms this hypothesis of the earth's figure by observations of the diminution of the simple pendulum upon apptoaching the equator; to which Dr. *Pound* adds the analogy the earth has with some of the other planets, as *Jupiter*, which sometimes appears oval, its least axis being that about which it performs its revolution.

This opinion of Sir *Isaac Newton* has likewise been maintained by M. *Huygens*, tho' with some small difference: But in 1691 M. *Eisenschmid* (in his diatribe de figura telluris elliptico-spheroide; ubi unà exhiberer ejus magnitudo per singulas dimensiones, consensu omnium observationum comprobata, Argentorati, apud Joh. Frider. Spoo, 1691. 4to. p. 54. cum fig.) having compar'd the admeasurements of the earth made in different latitudes, as that of *P. Riccioli* in *Italy*; of M. *Picart* in *France*, and of *Snellius* in *Holland*; and having found that the degree, which resulted from those different admeasurements, continued to become less in drawing nearer the poles, (which is quite the contrary of what follows from the earth's figure, supposed by Sir *Isaac Newton* and *Huygens*) M. *Eisenschmid* was thereupon of opinion that the earth was longer at the poles.

This opinion of M. *Eisenschmid* was afterwards confirm'd by the late M. *Cassini*, in the observations of the meridian of *Paris*: For, in 1701, having carried on these operations to the *Pyrenean* mountains, which is a space of above seven degrees and $\frac{1}{2}$, he found, that as he advanced to the south, these degrees increased $\frac{1}{870}$ part, or 72 toises each degree.

Since the meridian of *Paris* was in 1718 carried on northwards to the sea, M. *Cassini*, the younger, found, upon comparing more than eight degrees, which this meridian contains from sea to sea, that the increase, going northwards, was but from 60 to 61 toises each degree; as may be seen in the large treatise published in a separate volume, as a sequel to the *Memoirs of the Royal Academy of Sciences* at *Paris* for the year 1718. These reasons did not hinder Sir *Isaac Newton* from persisting in his first opinion of the figure of the earth being flatted at the poles, as appears in the 2d and 3d editions of his *Principia*, published in 1713 and 1726: And

it is very ſurpriſing, that by this very figure of the earth he demonſtrates a certain motion it has, to explain in the *Copernican* ſyſtem the preceſſion of the equinoxes, or the apparent motion of the fixt ſtars in longitude. Sir *Iſaac* finds the inequality of the degrees on the meridian, in ſo ſmall an extent as that of *France*, not ſenſible enough to be poſſibly determin'd by immediate obſervations; and he is of opinion, that we ought more to rely on the obſervations of the ſimple pendulum, and on the other principles which he has built upon, to conclude the earth flatted at the poles.

In 1720 M. *Mairan* attempted to reconcile the two different hypotheſes of Sir *Iſaac Newton* and M. *Caſſini*, by imagining that the earth, at its creation, being without motion was of a much more oblong figure than that which *Caſſini* thinks it has at preſent: So that it might have been reduced to that which it now has, by the diurnal motion on its axis, *&c.* But Dr. *Deſaguliers*, who is of Sir *Iſaac Newton*'s opinion, has made appear in *Phil. Tranſ.* N° 388, that M. *Mairan*'s ſuppoſition is contrary to the laws of motion; and he has moreover propos'd ſeveral conſiderable doubts on the obſervations and ſuppoſitions made uſe of by M. *Caſſini* in his determination of the earth's figure in 1718.

As ſoon as the meridian of *Paris* had been extended from one ſea to the other; and M. *Caſſini* had thence deduced a confirmation of the ſyſtem of the earth's being longer at the poles; M. *de L'iſle* deviſed a new method of deciding the queſtion, by the obſervation of the degrees of the parallel, compared with thoſe of the meridian

For that purpoſe he conſider'd, that as the degrees of the meridian and thoſe of the parallel, at the ſame elevation of the pole, had different relations, according to the different figures aſcrib'd to the earth; nothing more was requiſite for concluding which hypotheſis was the true one, than to determine this relation by immediate obſervation.

Having ſuppoſed that there had been obſerved on the parallel of *Paris*, a ſpace nearly of the ſame magnitude with that on the meridian, that is, of about 13 degrees; ſince that on the meridian is about 8 degrees and $\frac{1}{2}$; M. *de L'iſle* found by an exact calculation, that according to the figure M. *Caſſini* has given to the earth, this ſpace ought to contain 13 minutes and $\frac{1}{3}$ of the parallel more than in the hypotheſis of the earth's being ſpherical; which appeared to him conſiderable enough to be able to decide between theſe two hypotheſis, and by a

ſtronger

ſtronger reaſon, between the hypotheſis of Sir *Iſaac Newton* and M. *Caſſini*; ſince the difference ought to be ſtill more conſiderable than that now ſpecified.

M. *de L'iſle* concluded at leaſt, that independent of the figure of the whole earth, which could not be determin'd by the ſole obſervations made in *France* without making ſuppoſitions, and admitting principles which are ſtill liable to be conteſted; it would be of great conſequence towards conſtructing exact maps of the kingdom, to aſcertain this relation by obſervations, which conſiſted only in forming triangles along the parallel of *Paris*, and obſerving at the two ends the difference of the meridians, by the moſt exact methods.

The difference, he has now mention'd, ſeem'd to him to be ſo conſiderable, that he was in hopes of being able to determine it by means only of 2 places within ſight of each other, and ſituate to the eaſt and weſt; provided their difference of longitude were accurately obſerv'd, independently of aſtronomical obſervations, by means of lighted fires; after the manner that M. *Picart* put in practice in *Denmark*, for determining the difference of longitude of the aſtronomical tower at *Copenhagen* and *Uraniburg* in the iſle of *Huen*. With this intent, in *April* 1720, M. *de L'iſle* went ſome diſtance from *Paris* ſouthwards, to the places he judged propereſt for his purpoſe: But his deſign was not then executed, for want of aſſiſtance, and for other reaſons, he omits to mention.

Since that time M. *de L'iſle* ſaw with pleaſure, that S. *Poleni* had hit upon the ſame thought with him; as may be ſeen in his letter to the Abbè *Grandi*, dated *November* 1724.

The deciſion of this famous queſtion of the earth's figure had ſtopped here; when in 1733, the miniſter of *France* having thought it neceſſary to conſtruct an exact map of the whole kingdom; and being informed that the work could not be better carried on than by the aſtronomers of the *Royal Academy* of *Sciences*, applied to M. *Caſſini* on that head; who was of opinion, that in order to execute it with the utmoſt exactneſs, the ſame method ought to be made uſe of as for the meridian, by taking thro' the whole extent of the kingdom, triangles linkt together, by means of objects ſeen ſucceſſively one from another, &c. This project of making a map of *France* by ſuch triangles had been already offer'd to M. *Colbert* by M. *Picart* in 1681, but was not then executed. However M. *Caſſini* propos'd, that theſe triangles ſhould be begun in a direction perpendicular to the meridian, in order to render theſe operations

of

of ſervice towards the deciſion of the earth's figure, purſuant to the method ſpoken of above: And M. *Caſſini*, having in perſon undertaken theſe operations, and carried them that ſame year, *viz.* 1733, from *Paris* to St. *Malo*, whoſe longitude from *Paris* M. *Picart* had obſerv'd in 1681; the relations of the degrees on the meridian and parallel were found to be ſuch as were requir'd in the hypotheſis of the earth lengthen'd at the poles, and even more lengthen'd than *Caſſini* had determin'd in 1718: For, inſtead of the diminution of a ſixtieth part for each degree of the parallel, which M. *de L'iſle* had found according to the earth's figure, as determin'd by *Caſſini* in 1718, this latter deduced from his operations in 1733, a diminution of the 36th part of each degree.

True it is, that M. *Caſſini*, in the account he gave of this determination at a publick meeting of the *Royal Academy* at *Paris Nov.* 14. 1733, does not give it as entirely ſure; becauſe the longitude of St. *Malo*, with regard to *Paris*, was collected but from one obſervation only of *Jupiter*'s firſt ſatellite, wherein there may poſſibly be ſome error: But at leaſt M. *Caſſini* ſeems certain, that there is a very conſiderable diminution in the degrees of the parallel of *Paris*; which confirms his opinion of the earth's being longeſt at the poles. This we are likely to have a better certitude of hereafter, ſince this admeaſurement of the parallel of *Paris* is carrying on in *France* by M. *Caſſini*'s ſons, M. *Miraldi*'s nephew, and ſeveral other young mathematicians, inſtructed by M. *Caſſini* in this ſort of work.

It has been already ſaid, that all theſe operations performed in *France*, for the figure and magnitude of the earth, could not ſerve to determine the earth's figure out of *France*, without the aſſiſtance of certain hypotheſes; unleſs the ſame thing were undertaken and carried on in the other regions of the earth, more ſouthern and northern than *France*. It is upon this conſideration, that the *Royal Academy of Sciences* took up the reſolution of ſending ſome aſtronomers to make the like obſervations as near the equator and the poles as poſſible; which are the places where the difference of the degrees on the meridian ought to be the greateſt, according to the different hypotheſes.

In *April* 1735, three mathematicians and aſtronomers of the Academy, *viz.* M. *Godin*, *Bougher*, and *De la Condamine* ſet out from *France* for the province of *Quito*, the moſt northern part of *Peru* in *America*; in order to obſerve, juſt under the equinoctial line, the magnitude of ſome degrees of the meridian and equator.

As

As to the other mathematicians and aſtronomers of the ſame Academy, viz. M. *de Maupertuis*, *Camus*, *Clairaut* the younger, and *Monnier*, the younger, who have been ſent to the north, they departed from *France* in *April* 1736, with M. *Celſius*, profeſſor of aſtronomy at *Upſal*, who accompanied them as far as the bottom of the gulph of *Bothnia* in *Sweden*, where they might meaſure about a degree on the meridian at its croſſing the polar circle: But as by the laſt accounts M. *De L'iſle* had of them, they had not finiſh'd their operations; it is not yet known whether the magnitude of the degree meaſur'd by them, favours the opinion of M. *Caſſini*, or that of Sir *Iſaac Newton*: All we know is, that they have found the length of the ſimple pendulum favourable to the latter, that is, longer under the polar circle than farther ſouth. M. *De la Croyere* had already found the ſame thing: For, being at *Archangel* in 1728, he there obſerved, in the exacteſt manner he poſſibly could, the length of the ſimple pendulum, which he found to be $2\frac{3}{5}$ of a line longer than at *Paris*.

We are likewiſe informed by the other Aſtronomers gone to *Peru*, that in their way towards the equator, being at St. *Domingo*, in Lat. 18 degrees, 37 minutes, they there found the pendulum ſwinging ſeconds to be about 2 lines ſhorter than at *Paris*. Thus, all we hitherto know from thoſe Gentleman, on their expeditions to the north and to the line, confirms the opinion of Sir *Iſaac Newton*, and his adherents: And yet M. *Mairan* pretends, that this ſhortning of the pendulum in drawing nearer the equator, is in one ſenſe entirely independent of the earth's figure.

Thus it appears from the foregoing account, that the queſtion concerning the earth's figure is not yet decided: Nay, it is not impoſſible, that after finiſhing all the obſervations which are actually making, new difficulties may ariſe, and new objections be ſtarted, that may prevent its being entirely decided. However all this work cannot fail throwing a great deal of light on this important queſtion, and procuring conſiderable advantages to geography, aſtronomy and natural philoſophy.

It is with this view, and particularly to render ſuch important ſervice to the geography of *Ruſſia*, that M. *De L'iſle* thinks it neceſſary to undertake a work of that nature in *Ruſſia*; towards executing which, there are great advantages, which other nations have not. One of the principal, is the great extent of *Ruſſia* every way: For, were the meridian of the *Imperial Obſervatory* of *Petersburgh* to be determined, it might be carried

carried to between 22 and 23 degrees; which is a fourth part of the diſtance from the pole to the equator. The meridians of *Moſco* and *Aſtracan* are not of leſs extent; and conſequently, by the admeaſurement of ſome one of theſe meridians, might be determined more exactly, than could have hitherto been done, the inequality that ſubſiſts between the degrees of the meridian.

This is what the Great *Caſſini* wiſh'd for; when, after having in 1701, determin'd this inequality by the extent of 7 degrees obſerv'd in *France*, as mention'd above, he ſays, that this fact might be verified by menſurations of greater extent, if the other princes of the earth did contribute as much as the King of *France* towards the perfecting of ſciences.

In the great extent, which might be given to the meridian of *Petersburgh*, as aforeſaid, there would be the advantage of knowing, by operations linkt together, or uninterrupted, the magnitude of ſome degrees equal to thoſe which have been meaſur'd in *France*; and to that which the *French* aſtronomers have meaſur'd in *Sweden*; and not only all the degrees between the two, which the *French* aſtronomers have not had in their power to obſerve, but alſo ſome degrees farther northward than that meaſur'd by them in *Sweden*.

As the exigencies of geography require the triangles, taken for the determination of the meridian, to be continu'd on every ſide, and principally in directions perpendicular to the meridian, or according to the parallels: With what great exactneſs may we not then determine the proportion of the degrees on the parallels to thoſe on the meridian, by means of the vaſt extent of the *Ruſſian* empire; which on its weſtern ſide extending as far as all the dominions of *Europe* from the moſt northern to the moſt ſouthern, has no other bounds to the eaſt than the eaſt itſelf, ſo to ſpeak; ſeeing its extent that way contains near half the earth?

Another great advantage to be obtain'd by the work M. *De L'iſle* now propoſes to be made in *Ruſſia* is, that coming after others, the benefit of all their knowledge and experience in the like kind of admeaſurements will be reaped: Whence one may expect to ſucceed, and execute it better than could have been done elſewhere, by applying timely remedies againſt the difficulties that occurr'd in other places.

Theſe operations are to be founded on a baſis of the greateſt length poſſible; which muſt be actually meaſur'd, and with the greateſt exactneſs that may be; as it is to ſerve as a foundation

tion to the admeaſurement of all the triangles: And in this point too there is a very great conveniency near *Petersburg*, ſeeing on the ice there may be meaſured out a baſis, greater than has been hitherto taken; namely from the coaſt of *Ingria* about *Peterhoff* to the coaſt of *Finland* towards *Syſterbeck*. There is no leſs than 20 werſts diſtance between theſe 2 extremities; and this great diſtance may be meaſur'd very exactly, this year eſpecially, *viz.* 1737, that the ice is very even: Moreover, as this baſis is ſituate between the iſle of *Cronſtad* and *Petersburgh*, in a direction nearly perpendicular to the diſtance from *Petersburgh* to *Cronſtad*; there can be no better method for inferring thence, by exact obſervation of the angles taken at the extremities of this baſis, the diſtance from the centre of the *Imperial Obſervatory* to the ſteeple of the New Church of *Cronſtad*; which 2 objects are ſeen reciprocally from each other, and are not leſs than 30 werſts aſunder: And this diſtance once known exactly will ſerve as a foundation for all the triangles that are to be taken; of which each of the ſides may have not leſs than from 30 to 40 werſts, according as objects ſhall be found advantageouſly ſituated for that purpoſe. There is the mountain of *Donderhof* to begin with, which, with the *Imperial Obſervatory*, and the ſteeple of *Cronſtad* Church, forms one of the moſt convenient triangles imaginable for the ſubject propoſed.

In taking obſervations at theſe 3 places may be ſeen, if others of the ſame advantageous ſituation can be diſcover'd: But when no remarkable objects are found of the ſituation and diſtance ſought for, they muſt be erected on purpoſe, in the ſame manner as was of neceſſity done in other countries. And this may be done in *Ruſſia* with more eaſe; ſince, in places where the woods intercept the ſight, ſmall towers may be rais'd, at very little expence, out of theſe ſame woods, with ſignals placed on them, which may be ſeen as far as is required. In open places, where conſequently wood is not ſo common, ſignals alone, without towers, will ſuffice.

The moſt neceſſary inſtruments for executing this undertaking, are, beſides the ordinary aſtronomical inſtruments, a common quadrant of between 2 and 3 foot radius, for obſerving the angles of the triangles that ſhall be taken; and a portion of a circle of the greateſt radius that can be conveniently had, for obſerving the arches of the heavens correſponding with the diſtances meaſur'd on the earth.

The quadrant ought not to have a radius of more than between 2 and 3 foot: For, if it be bigger, it cannot for the moſt part be made uſe of in ſteeples and other places of conſiderable height, where it is requiſite to obſerve: But alſo if it be leſs than 2 foot, it will not give the quantity of the angles with ſufficient exactneſs.

As to the other inſtrument for obſerving the arches of the heavens, its radius ought not to be leſs than from 12 to 15 foot; but it is not neceſſary that it ſhould contain a large portion of a circle: It is only requiſite to have this portion ſomewhat larger than the arch of the heavens intended to be meaſur'd. Thus, as the meridians, which may be traced in *Ruſſia*, can be extended but between 22 and 23 degrees, as already mention'd, it will ſuffice, that the inſtrument employ'd therein be a portion of a circle of 30 degrees.

M. *Picart*, for his firſt operation, got an arch of a circle made of 18 degrees and of 10 foot radius, with which he thought himſelf ſure within 2 or 3 ſeconds: And no other inſtrument was made uſe of in the chief obſervations for the meridian of *Paris*. The aſtronomers who are gone to *America* carried with them an inſtrument of 12 foot radius, and of a portion of a circle of 30 degrees. But thoſe come to *Sweden* contented themſelves with a portion of a circle of 5 degrees and $\frac{1}{2}$, and 9 foot radius: But this inſtrument made by Mr. *George Graham*, a very able mechanic in *London*, is by its conſtruction ſo exact, that the aſtronomers who have us'd it, think themſelves ſure to 2 ſeconds. The one for the obſervations in *Ruſſia* ſhould be made by the ſame artiſt, and of the ſame conſtruction.

It is with ſuch an inſtrument that Mr. *Bradley*, a celebrated *Engliſh* aſtronomer, has diſcover'd in the meridian altitudes of ſome fixt ſtars, certain conſtant and annual variations, which do not proceed either from the variation of the refractions, or from the parallax of theſe ſtars; or in fine, from any nutation or wavering of the earth's axis; but which he accounts for by the ſucceſſive motion of light.

Whatever be the cauſe of theſe variations, (which cauſe, as well as its effect are not hitherto, perhaps, entirely clear'd up) as they may poſſibly happen in the ſpace of time requiſite to be ſpent in making the obſervations for the meridian, or in paſſing from one end of the meridian to the other; it is neceſſary with the ſame inſtrument, or ſuch another that is of pretty near the ſame exactneſs, to examine the variations

tions of the ſtars made uſe of. Wherefore it would be of conſiderable advantage, not only for the obſervations of the admeaſurement of the earth, but alſo for all the other principal reſearches in aſtronomy to have orders given for procuring two mural quadrants of Mr. *Graham*'s making, and of the ſame conſtruction, as has been alrealy ſpecified; for which there are already walls raiſed at the *Imperial obſervatory*, in the plane of the meridian.

With theſe two quadrants, which might be of ſeven foot radius, and the moveable teleſcope 9 or 10 foot long, obſervations of the utmoſt accuracy might be made, ſuch as the preſent ſtate of aſtronomy requires.

Beſides theſe inſtruments now mentioned, which are abſolutely neceſſary to a ſolid eſtabliſhment of aſtronomy and geography in *Ruſſia*, there are ſtill ſome other ſmaller inſtruments, that may be of conſiderable uſe in the operation propoſed, or that may ſerve to make other curious and uſeful obſervations at the ſame time, that thoſe for the admeaſurement of the earth are making.

When the ſides of the triangles, taken for meaſuring the earth, terminate at very elevated places, as on the tops of the higheſt mountains, it is neceſſary to reduce theſe triangles to what they would be, had they been obſerv'd in horizontal planes ſituate upon a level with the ſea. For this purpoſe muſt be known the height of the mountains above the level of the ſea, which cannot always be determined geometrically, or would at leaſt be too tedious to perform: Wherefore, in the meridian of *Paris*, which croſs'd very high mountains, M. *Caſſini* was of opinion, that he ought to fix their height by a ſhorter method, which is that of the height of the ſimple barometer, obſerv'd on the top of each mountain, and compar'd with that obſerv'd at the ſame time in another place, whoſe elevation above the level of the ſea was known. But as that method ſuppoſes the knowledge of the proportion, which the different fallings of the mercury, keep with the different heights to which the barometer is carried; and as natural philoſophers are not hitherto entirely agreed on this head, for want of obſervations of ſufficient accuracy: Hence it happened, that Dr. *Deſaguliers*, making appear that M. *Caſſini* has not made uſe of the moſt exact proportion, found reaſons for correcting, or at leaſt for doubting of ſome of *Caſſini*'s calculations. Thus it muſt be by the aſſiſtance of new experiments, better circumſtantiated than thoſe hitherto made, and purſuant to a

theory entirely agreeing with these experiments, that this method may be employ'd with certainty, for determining the height of mountains by the barometer, and reducing the angles obsery'd from the tops of these high places, to what they would be, if they had been obsery'd on a plane horizontal with the level of the sea: Now these new observations may be made on the way in tracing the meridian; and for that purpose M. *de L'Isle* begun to construct compound barometers, which, by the peculiar nicety of their make, will serve with accuracy to observe the quantity of the mercury's fall, at the different elevations, to which they shall be carried, in order to fix with greater certainty the proportion of that fall: And in the construction and use of these instruments he proposes to provide against the effects of heat; which, as it is different in the different times and places of making these experiments, may possibly produce apparent variations, of which it is necessary to keep an account.

There is still another method of determining the elevation above the level of the sea of all the points, in which the triangles terminate, that are made for the admeasurement of the earth: And this may be done by beginning these operations near the sea, and actually measuring how many toises and feet the planes of the first stations are elevated above the level of the sea: For, if the angles of the apparent elevation of the second stations, seen from the first, be afterwards obsery'd, it will be an easy matter, from the known distances, to reduce the true elevations of the latter above the former; and consequently above the level of the sea, making proper allowances in the calculations for the difference of the apparent level from the true one: In this method nothing is to be apprehended but the variation of refractions; but for this a remedy may be found, for the most part, by returning upon one's steps, that is, by reciprocally obsery ing the first stations seen from the second: For, if it be found, that as much as the second station appears elevated above the first; so much the first is depressed below the second, except the small difference which must arise according to the given distance, it will be a proof that the refraction has been of no prejudice.

The observations and determinations of the true heights of all the places, which are to be visited, will not be the least laborious of those that are to be made in these journies; but their usefulness will be a sufficient recompence for the trouble; since they will afford us the means of knowing all

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the chief unevenneſſes of the ground travers'd by theſe great triangles; which being compar'd with the length of the courſe of the rivers, may give us room to judge of their rapidity, of the eaſe or difficulty of their communications, &c.

The other conſiderable obſervations and experiments to be made in the journies, undertaken for ſuch enquiries, are the obſervations of the magnetic needle, both as to its dip and variation: But chiefly the obſervations of the length of the ſimple pendulum; which at preſent is become requiſite to be obſerv'd with as much exactneſs and in as many places as poſſible.

All the operations and obſervations here propos'd, however arduous and difficult they may prove, have no other end than the benefit of geography: thoſe who are to have the management of this enterprize muſt be attended by ſeveral ſurveyors and other mathematicians of *Ruſſia*, who are to be inſtructed on the road, and employ'd at the ſame time in leſſer operations with ſmaller inſtruments: By which means the maps of the countries, taken in by theſe great triangles, may be verified. And thus according as this work advances, the finiſhing ſtroke may be given to the maps of *Ruſſia*.

The actual Menſuration of the Baſis propoſed in the preceding Diſſertation: by the ſame. Phil. Tranſ. N° 445. p. 50.

M. *De L'Iſle* actually undertook to meaſure the baſis mentioned above, and he had the good fortune to meaſure it exactly on the ice, by taking the preciſe diſtance between the caſtle at *Peterhoff*, and the caſtle of *Doubki*, ſituate oppoſite to it on the *Finland* coaſt. He found the diſtance between the oppoſite walls of theſe caſtles 74, 250 feet *Engliſh*. This baſis being much greater than any of thoſe hitherto employ'd for this purpoſe, gives room to expect great exactneſs in the whole work, when it ſhall be carried on in the ſame manner: It will at the ſame time ſerve to make a very exact map of the bottom of the gulph of *Finland*.

It is for the ſame purpoſe, and for better ordering the charts of the coaſts of the *Baltic*, that he intended (as ſoon as his project ſhall be approved in its full extent) to begin to meaſure his triangles along the coaſts of *Ingria* and *Livonia*, to the iſlands of *Dagho*, *Oeſel*, &c. And to the end that the charts of the places taken in by theſe triangles might be

finiſhed

finiſhed at the ſame time, he deſign'd to take with him all the charts of theſe parts, which could be had, in order to verify and correct them in his way.

He likewiſe intended to publiſh, as ſoon as poſſible, all the operations and obſervations he ſhould make in his expedition; that thus, early benefit might be reap'd from them; and that the publick at the ſame time the charts come out, might be acquainted with the foundation on which they are conſtructed. What retarded the publication of the whole detail of his operations in taking the baſis, that is, of the precautions he uſed in aſcertaining it, was his employing *Engliſh* feet in meaſuring, which he had a mind to reduce tot he *Ruſſian* meaſure; but as it was requiſite to conſult the original ſtandards on this head, which he had not hitherto been able to procure, he was for theſe reaſons obliged to delay the publication of theſe firſt obſervations.

Obſervations of two Parhelia, or mock-ſuns, ſeen December 30. 1735; *as alſo of an Aurora Borealis,* December 11. 1735. *by Mr.* Timothy Neve. Phil. Tranſ. N° 445. p. 52.

ON *Tueſday* the 30th of *December*, 1735. betwixt *Cherry Orton* and *Alwalton* in the county of *Huntingdon*, he obſerved two parhelia; the firſt of which ſhone ſo bright, that at firſt he took it for the real ſun; till looking a little farther on his left hand, he ſaw the true ſun, much the brighteſt, in the middle, and a mock-ſun on each ſide, in a line exactly parallel to the horizon. He gueſs'd their diſtance to be about 40 diameters of the ſun, or as they uſually appear 23 degrees. That on the left hand of the ſun, when he ſaw it firſt, was ſmall and faint; but in about two minutes time it became as large and bright as the other, and appeared at once like two white lucid ſpots on each ſide of the ſun, eaſt and weſt, ſeemingly as big, but not ſo well defined: In about three minutes they loſt both their colour and form, and put on thoſe of the rainbow; the red and yellow in both were very beautiful and ſtrong next the ſun, the other colours fainter. They became as two parts of an arch, or ſegment of a circle, with the concave part towards the ſun, only round at top, the light and colour ſtreaming downwards, and tending towards a point below: This continued for four or five minutes, when the colours gradually diſappearing, they became, as before, lucid ſpots, without any diſtinc-

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tion of colours: They laſted a full hour; ſometimes the one brighter, and ſometimes the other, probably according to the variation of the clouds and air. When Mr. *Neve* firſt ſaw it, it was exactly a quarter after 11. There had been a froſt in the morning, which went away pretty ſoon with a thick miſt; and between 10 and 11 o'clock it clear'd up, leaving only a hazineſs in the air behind it: The weather quite calm, wind, as he thought, at N. W.

Theſe *parhelia* are commonly ſeen with a circle or halo round the ſun, concentrical to it, and paſſing thro' the diſks of the ſpurious or mock-ſuns: But there was not the leaſt appearance of ſuch a circle here; it having only a tendency towards one, when it was ſeen with the rainbow colours.

The other phenomenon was that pretty common one of the *Aurora borealis*; of which tho' there are ſo many exact and curious accounts in the *Philoſophical Tranſactions*; yet Mr. *Neve* does not remember any one in the manner he ſaw this of the 11th of *December* 1735.

A little after 5 o'clock, he obſerv'd the northern hemiſphere to be obſcur'd by a duſky red vapour; in which, by degrees there appear'd ſeveral very ſmall black clouds near the horizon. He thought it ſeem'd to be a preparation for thoſe lights, which were afterwards ſeen; the firſt eruption of which was within a quarter of an hour, full eaſt from behind one of the ſmall dark clouds, and ſoon after ſeveral others full north: Theſe ſtreams of light were of the ſame duſky red colour with the vapour, they firſt appear'd and vaniſh'd inſtantly. He obſerv'd eight or ten of theſe at once, about the breadth of the rainbow, of different heights, ſeveral degrees above the horizon, and they look'd like ſo many red pillars in the air; and no ſooner did they diſappear, than others ſhew'd themſelves in different places: In about half an hour, this colour of the vapour gradually changed itſelf towards the uſual white, and ſpread itſelf much wider and higher; and after that, appear'd as uſual.

Obſervations of two Parhelia *ſeen at* Wittemberg *in* Saxony *on* Dec. 31, 1735, O. S. *by M.* Weidler. Phil. Tranſ. N° 445. p. 54.

DEC. 31, 1735. O. S. a little after 10 in the morning, a friend told M. *Weidler*, that ſeveral ſuns were to be ſeen in the heavens: Upon this the latter went directly into the garden adjoining to his houſe, and he immediately ſaw, near the ſun

ſun S (Fig. 15. Plate IX.) on his left or weſtern ſide, the following particulars; namely the parhelion B, as big as the true ſun. This mock-ſun was amidſt ſmall, round, white, clouds, ſet thick and cloſe to one another: The part of the parhelion, which faced the weſt, was not round, but broken, having about a third part of its circumference open, and ſhooting out the long bright ſtream or tail B H: To this ſtream, both above and below, adher'd another ſtream F G, ſomewhat incurvated, with its horns turned from the ſun weſtwards: The middle of this mock ſun ſhone with ſo great a light, that the naked eye could not bear it; wherefore, he view'd it attentively thro' a glaſs darken'd with the ſmoke of a wax-candle: The light of the parhelion B appear'd much weaker than that of the true ſun; its circumference, which faced the ſun, was red; in like manner that part of the ſtream F G, which was towards the ſun, was purple. Within the red border appear'd the other colours of the rainbow, as yellow, green and azure: And the ſtream B H was likewiſe embelliſh'd with red and yellow. Both edges of this were reddiſh, and its middle yellowiſh. The ſun S was 15 degrees and $\frac{1}{2}$ above the horizon; and its image B was near the ſame altitude: For, he then found it to be 14 degrees. He meaſur'd the diſtance from S to B, more than once, and he found it to be 20 degrees. The arch F G was near 6 degrees in length. Moſt of the ſouth part of the hemiſphere was overcaſt with white clouds, interſperſed here and there with ſome darker ones: There were ſome thin clouds before the true ſun, thro' which his rays eaſily paſſed: When thicker clouds ſurrounded the ſun, the brightneſs of the parhelion was leſſen'd: The parhelion was now and then hid by dark clouds. The thin white clouds, with which the northern part of the ſky was overcaſt, reach'd up to the zenith. Soon after his firſt obſerving the parhelion B, as he look'd up to the zenith, he ſaw the beautiful rainbow C D E parallel to the horizon, with its horns turned to the north: It had the uſual colours of the rainbow, all very diſtinct; the purple was on the ſide facing the ſun; next to it was the yellow, then the green, and laſt, the azure. A line, drawn from the ſun's centre to the middle D of the *iris*, tended to the zenith, and was a portion of that vertical circle, in which the ſun then was. The point D was 61 degrees diſtant from the horizon; wherefore, the diameter of the rainbow was 58 degrees: However, there was but part of the rainbow C D E ſeen; the ends of which were ſometimes but 38 degrees from each other: For, more or leſs of it appear'd at different

times;

times; but ſcarcely above a fourth part of its circumference at any time. It was ſometimes ſeen among ſmall white clouds, which were about the zenith, and ſometimes in a clear ſky. It laſted till the ſun and moſt part of the ſky was overcaſt by thick clouds. The thickneſs of the rainbow CK, as well as M. *Weidler* could eſtimate by the naked eye, was one degree of a great circle.

But as the neighbouring houſes hinder'd his having a free proſpect eaſtwards from his garden, he went to another place, whence he had a full view of the hemiſphere: And having reach'd thither a little before 11, he immediately ſaw another parhelion A to the eaſt, 20 degrees from the ſun, as the foregoing was, and elevated 15 degrees above the horizon. This mock-ſun was not inferior to the other B in brightneſs: For, the naked eye could no more bear it than that; its light was white, its figure round, and its ſize equal to that of the ſun S. This parhelion A ſhot out the ſtream IL, which was rectilinear, white and reſplendent, 8 degrees in length; and as far as he could diſcover, void of colours: For, it appear'd among ſmall, white, broken, clouds; and laſted ſomewhat longer than the former, without changing its figure. Upon the ſun's being hid by thick clouds about half an hour after 11, both theſe mock-ſuns diſappear'd; but became viſible again, upon the ſun's ſhining bright.

The whole of the phenomena, obſerv'd in theſe parhelia, amounts to this; that the true ſun S was accompanied by 2 parhelia; both 20 degrees diſtant from the ſun, one on each ſide; and having nearly the ſame altitude with the ſun from the horizon. Above the parhelia, part of a rainbow ſurrounded the zenith; and each of the parhelia ſent forth a bright luminous ſtream or tail; one rectilinear and white, the other ſomewhat incurvated and colour'd. Moreover, from the weſtern parhelion, a ſtream parallel to the horizon, and ſomewhat pointed, extended itſelf on the ſide oppoſite to the ſun; and this ſcene laſted for the 2 hours of 10 and 11 before noon, till thick clouds put an end to it. There was no appearance of an entire *crown*, ſuch as uſually accompanies parhelia, and encircles the ſun; tho' M. *Weidler* obſerved the tract of the ſky near the ſun, both with the naked eye and thro' glaſſes.

As to the ſtate of the heavens on the 31. of *December*, when the parhelia were obſerv'd: Early in the morning a thick fog overſpread the horizon: About 9 o'clock this fog condenſed into ſmall drops of rain, which fell ſlowly: Soon after, the vapours

were collected into thin clouds, particularly in that part about the ſun: Then the ſky became clear about the north; and there blew a gentle wind a little to the ſouth of the eaſt.

After noon, clouds gather'd to the weſt: About 30 minutes after twelve, the whole hemiſphere was overcaſt but in the evening it became clear and ſerene on all ſides. On the following days, from the 1ſt to the 6th of *January*, the ſky was conſtantly cloudy, or dark and the ſun ſeldom ſeen thro' the breaks of the clouds. On the 7th, the weather clear'd up, which laſted 3 days. On the 10th, the whole hemiſphere was overcaſt with clouds; and therefore this appearance of parhelia has not been attended with any uncommon weather.

The publiſher having ſent M. *Weidler* an account of Mr. *Neve*'s obſervation of the mock-ſuns, ſeen by him in *England*, which ſeem to agree in ſo many circumſtances with thoſe ſeen by the other in *Germany*; M. *Weidler* in his anſwer ſaith: 'That it ſeems to him very worthy of remark, that parhelia 'ſo very much alike, ſhould appear two ſubſequent days in 'places ſo diſtant from each other; which indicates a ſimilar 'ſtate of the air or atmoſphere in both.' This extraordinary incident put M. *Weidler* upon writing an eſſay on the cauſe of parhelia: And accordingly he hath publiſh'd a ſmall pamphlet in *quarto*, intitul'd *Jo. Frid. Weidleri* commentatio de *parheliis, Menſe januarii* Anno, 1736, prope *Petroburgum Angliæ & Vitemburgæ Saxonum* viſis. Accedit de rubore cæli igneo menſe *Decembri* Anno 1737 obſervato *corollarium*, *Vitembergiæ*, 1738, in quarto.

An Obſervation of three Mock-ſuns *ſeen in* London, September 17, 1736; *by Mr.* Folkes. Phil. Tranſ. N° 445. p. 59.

SEPT. 17, 1736, a little after 7 o'clock in the morning, as Mr. *Folkes* was reading in a room that looks towards the north-eaſt he accidentally took notice of an odd ſtream of colour'd light, ſhooting upwards from the ſun, as he then thought, which ſhone thro' a thin wateriſh cloud: But recollecting that the appearance was ſeveral degrees more northerly than the ſun's true place at that time, he immediately went to the window, and found what he had taken for the ſun was a parhelion ſhooting out a ſhort horizontal ſtream or tail towards the north; the ſun itſelf ſhining pretty bright and clear at the ſame time. He alſo obſerv'd that the ſtream he had at firſt ſeen was part of an arch concentric to the ſun, and paſſing thro' the parhelion: This arch was for a good way tolerably defin'd, and tinged

inged with red on the inside, and a bluish white on the other. He then cast his eye to the other side the sun, where he observ'd a 2d parhelion, at the same distance from him towards the south, tho' not yet so bright as the first. He then went up to his leads, his prospect being too confined below; where soon after he was come, he found the phenomenon to improve considerably, the arch round the sun forming itself into more than a semi-circle, reaching almost to the horizon northwards and with very little discontinuance beyond the second parhelion towards the south. He then began to perceive a 3d parhelion, where the circle surrounding the sun, would have been cut by the vertical passing through him; and in the same place his circle was touched by the arch of another, in some sort confounding itself with it in the place where the 3d parhelion appear'd: This was a good deal fainter than the other two; and the last mentioned arch extended but a little way: So as to be difficult to determine where its centre lay; this arch was likewise colour'd, but with red on its convex part: He had some time before this likewise began to see another circle, surrounding the sun at the distance of about 45 degrees, which appear'd to be about twice the distance of the first; and this likewise encreasing whilst he was considering it, became little less than a semicircle, being likewise tinged with red like the other on the inner side. When this circle had thus pretty well formed itself, he also discover'd the arch of a 4th circle, touching this, or rather confounding itself with it in its highest part, and surrounding, as it seem'd, the zenith. Of this last circle he saw, when it was most compleat, better than half, and it was much stronger colour'd than any of the others, being of a bright red on its convex, and a good blue on its concave part. In the part where this circle blended itself with the larger of those that were concentric to the sun, their common part was nearly white, and brighter than the rest, tho' hardly enough, to call it a 4th parhelion. All which appearances are represented in Fig. 16. Plate IX. The principal mock suns continu'd tolerably bright till near 8 o'clock, the southern part of the phenomenon improving as the northern part decay'd; and the southern parhelion was once so bright, that taking the advantage of a place where a chimney intercepted the true sun, it cast a very visible shadow. In like manner the white and luminous horizontal tail, that proceeded from this parhelion was much longer than that of the other, reaching at one time beyond the outer of the two concentric

circles. The parhelia themſelves, tho' very luminous, were however never defin'd with any exactneſs as to their diſks, but look'd as the ſun is ſometimes ſeen thro' a thin whitiſh cloud; and they were themſelves of a reddiſh colour on that ſide next the true ſun. About 8 the phenomenon was ſenſibly decreas'd, and had entirely diſappear'd by 20 minutes after.

A rupture of the Ileum *from an external Contuſion without any external Wound*; *by* M. Chriſtianus Wolfius. Phil. Tranſ. N° 445. p. 61. *Tranſlated from the* Latin.

A Certain labouring man ended his days by a melancholy accident, as follows; a ſtone falling down upon his lower belly, ſtruck it ſo, that the place affected came indeed to view, but without any external wound. The day following about noon, the man, tho' pretty robuſt, contrary to all expectation, expired. Upon opening the abdomen, there was diſcover'd a large rupture in the *ileum*; ſo that it only adhered behind to the other guts with its contents diſcharg'd into the cavity of the abdomen. The liver was pale, quite void of its native colour; and even the lungs themſelves had receded from their natural ſtate, having loſt their natural colour. From the livid ſpot on the abdomen, M. *Wolfius* gather'd, that the ſtone had ſtruck with its ſharp end upon the belly, and that the gut was burſt by too great a tenſion, in the manner as incurvated bodies are broken in their ſuperior convexity.

Some new ſtatical Experiments; *by Dr.* Deſaguliers. Phil. Tranſ. N° 445. p. 62.

WHEN a long and heavy body lying on the ground is to be rais'd up at one end (like a lever of the 2d kind) while the other end keep its place, and becomes the centre of its motion; the prop made uſe of to ſupport it at any point in its whole length, ſuſtains a certain preſſure from the beam. Now the Doctor's experiments are to ſhew, by a force drawing always in the direction of the prop, what is the quantity of the preſſure on the prop, according to the length of the prop, the angle it forms with the beam, or with the horizon; and the diſtance from the centre of motion of the beam at which the prop is applied: For, when the prop is taken away, the force drawing in the direction of the prop will keep the beam *in æquilibrio*; and a force ever ſo little ſuperior to the friction, added to the power, will make it overpoiſe

poiſe the beam, and raiſe it higher; but overcome the power and bring down the beam, if it be added or applied to the beam.

Tho' in every caſe or experiment we have this analogy taken from mechanical principles, *viz.* that the

Intenſity of the power:
Is to that of the weight: :
As the diſtance of the line of direction of the weight:
Is to the diſtance of the line of direction of the power.

Yet to find thoſe diſtances nicely in the ſeveral applications of the prop, we muſt have recourſe to geometrical conſtructions and reaſonings. With theſe and the algebraical expreſſions of the ſame, the experiments exactly agree.

All the Doctor propoſes now is to ſhew the experiments by means of a machine which he contrived for the purpoſe, and got executed with great nicety, not in ornaments, but only where nicety in a mechanical inſtrument ought to be obſerv'd.

In this machine, the iron bar, or parallelipiped repreſenting the heavy body, weighs 12 drachms, 12 penny-weight, 12 grains, or 6060 grains; and its centre of gravity is at the diſtance of 20 inches and $\frac{1}{2}$ from its centre of motion.

The props he makes uſe of are, the one of five, and the other of ten inches: To overcome the friction, allow'd for by certain rules in all caſes, he uſes a nice braſs pully of three inches in diameter, whoſe pivots are but $\frac{75}{1000}$ parts of an inch in diameter: So that the 60th part of the power, added to it, will, in all caſes, overcome the friction.

Caſe 1. In which the prop is perpendicular to the horizon, exemplified by two experiments.

Exp. 1. The prop is equal to five inches, and placed under a point in the bar 10 inches diſtant from the centre of motion. Here the power acting in the direction of the prop, able to keep the bar in that ſituation, or the preſſure ſuſtained by the prop, will be found 250 ounces, 17 penny-weight, 15 grains; and the friction 8 penny-weight, 15 grains. The foot of the prop is to be at eight inches and $\frac{46}{100}$ from the centre of the motion.

Exp. 2. If the ſame prop of five inches be placed under a point in the bar at 30 inches from the centre of motion, the power or preſſure will be eight ounces, 12 penny-weight,

13 grains; and the friction equal to two penny-weight, 21 grains. The foot of the prop is to be distant from the centre of motion 29 inches $\frac{50}{100}$ parts.

Case 2. In which the prop is perpendicular to the bar, exemplified by three experiments.

Exp. 1. Now let the prop (still five inches long) be placed so as to be perpendicular to the bar in a point 12 inches distant from the centre of motion. Here the power expressive of the pressure should be 19 ounces, eighteen penny-weight, 4 grains; and the friction six penny-weight, 15 grains: But on account of a correction necessary to be made to this (because the bar is thick as well as heavy, and the centre of gravity above the surface to which the prop is applied) the power or pressure sustained will be only 19 ounces, 15 penny-weight, five grains; and the friction six penny-weight, 14 grains.

N. B. The distance of the foot of the prop in this case is 13 inches from the centre.

Exp. 2. The prop here is 10 inches long (still perpendicular to the bar) under a point in the bar, 24 inches distant from the centre: The power equal to the pressure sustained should be (if the bar were only heavy and not thick) nine ounces, 19 penny-weight, four grains; the friction three penny-weight, 11 grains and $\frac{1}{2}$; but with the proper correction, which shall be explained hereafter; it must be only nine ounces, 17 penny-weight, 15 grains; the friction seven penny-weight, seven grains. Here the foot of the prop is to be 26 inches from the centre.

Exp. 3. If the end of the prop be placed under a point in the bar; so that the horizontal distance of the foot of the prop be exactly equal to the distance of the centre of gravity from the said centre of motion, *viz.* 5. 20, inches; the power or pressure sustained by the prop will be precisely equal to the weight of the bar; *viz.* 12 ounces, 12 penny-weight, 12 grains. In this case the prop is distant from the centre of motion on the bar 9. 17, nine inches, and the friction four penny-weight, five grains.

Case 3. In which the angle formed by the prop with the horizontal line is given, either acute or obtuse.

As this case is very intricate (on account of the several powers of the sine and cosine of the given angle, which are multiplied into the prop, and into the weight of the beam) it is exemplified only in one experiment; namely, when the angle

gle, formed by the prop with the horizontal line, contained between the foot of the prop and the centre, is acute; then there is a *maximum* of preſſure, which the Doctor ſhews by experiment to be the very ſame with that given by the calculation. He ſuppoſes the angle formed by the prop and the horizontal line to be 60 degrees: The calculation of this *maximum* ſhews, that if the prop be 10 inches long, the diſtance meaſured upon the bar, to which the upper end of the prop muſt be applied, will be 10 inches $\frac{96}{100}$ parts, the bar itſelf forming then an angle of about 52 degrees, 12 minutes; and the horizontal diſtance between the centre of motion and the foot of the prop is then 11 inches $\frac{72}{100}$ parts.

N. B. Three things are to be remarked in this caſe.

1. That when the angle, made by the prop and horizontal line, contained between the centre of motion and foot of the prop, is acute, as in the laſt experiment, there is always a *maximum*: Whereas if the ſame angle were obtuſe, there would be no poſitive *maximum*: For, then the preſſure would continually increaſe, the nearer the prop is to the centre of motion.

2. That when the angle of the prop with the horizon is acute, as in the laſt experiment, the bar, or long and heavy body, can be rais'd, by applying the power or prop always with the ſame angle to the horizon, quite up to a vertical ſituation.

3. That the firſt caſe, which is when the prop is perpendicular to the horizon, is only a particular caſe of this more general one.

Caſe 4. Is when the angle, made by the prop with that part of the beam, contain'd between the point to which it is applied, and the centre of motion, is given either acute or obtuſe.

As the expreſſion of the power in this caſe is fully as intricate as in the laſt, the Doctor gives only one example or experiment; and for the greater ſatisfaction of thoſe who ſee it, he choſe that wherein the preſſure is in its *maximum*. He ſuppoſes, as before, the angle made by the prop (ſtill 10 inches long) with that part of the beam contained between the point to which it is applied, and the centre of motion, to be acute and of 60 degrees; then the *maximum* of preſſure will be, when the part of the beam intercepted between the centre of motion and the upper end of the prop is 12 inches $\frac{24}{100}$ parts; the bar is then elevated about 50 degrees 13 minutes; and the horizontal diſtance between the centre of motion

motion and the foot of the prop is then 11 inches $\frac{47}{100}$ parts.

N. B. The ſame things are alſo to be remark'd in this caſe as in the preceding.

1. If the angle made by the prop and the part of the beam, intercepted between the point of application and the centre of motion, be acute, there will always be a *maximum*. The contrary, if that angle be obtuſe.

2. If the angle be acute, the bar cannot be rais'd up to a vertical ſituation by applying the power or prop conſtantly with the ſame acute angle; but it may be rais'd quite up, if the angle of the prop with the beam be obtuſe.

3. The 2d caſe is but a particular caſe of this general one.

An Account of the Peruvian *or* Jeſuits Bark; *by* Mr. William Arrot. Phil. Tranſ. N° 446. p. 81.

THE tree from which the *Jeſuits Bark* is cut (Mr. *Arrot* himſelf having gather'd it) grows in the kingdom of *Peru*, in the *Spaniſh Weſt-Indies*; and moſt commonly found in the provinces of *Loxa*, *Ayavaca* and *Quenca*, ſituate between two and five degrees of ſouth latitude. This tree is tall, and has a trunk rather bigger than a man's thigh, tapering from the root upwards, has no boughs or branches till near its top; and theſe grow as regular as if lopp'd artificially, and with the leaves form exactly the figure of a hemiſphere: Its bark is of a blackiſh colour on the outſide, and ſometimes mixt with white ſpots, from whence commonly grows a kind of moſs, which the *Spaniards* call *barbas*; its leaves reſemble much the leaves of our plum-tree, of a darkiſh green colour on their upper or concave ſide, and reddiſh on their lower or convex ſide; its wood is as hard as our common *Engliſh* aſh, and rather tough than brittle.

There are 4 ſorts of the bark of this tree, to which the *Spaniards* give the following names, *viz. Caſcarilla colorada*, or reddiſh bark; *amarylla*, yellowiſh; *creſpilla*, curling; and *blanca*, whitiſh: But Mr. *Arrot* could only find 2 different ſorts of the tree; and he believes that the other 2 ſorts of the bark are owing to the different climates where it grows, and not to a different ſpecies of the tree. The bark called *colorada* and *amarylla* is the beſt, and differs from the *blanca* in this, that the trunk of the former is not nigh ſo big as that of the latter, the leaves being as deſcrib'd above: Whereas thoſe of the *blanca* are larger, and of a lighter green

green colour, and its bark has a very thick ſpongy ſubſtance, whitiſh on the outſide, and is ſo tough, that it requires the force of an ax to ſlice it from the tree: It is true, it is as bitter when cut down as the beſt ſort, and has then the ſame effect in intermitting fevers; but when dry and long kept, turns quite inſipid and good for nothing: And it is to be obſerved, that both ſorts have a much ſurer and quicker effect in cures, when green, than when dry. As the bad ſort is in great plenty, and the beſt very ſcarce and hard to be come at, large quantities of it are yearly cut, and ſent with a little of the fine bark to *Panama* for *Europe*.

The tree of the *creſpilla* is the ſame with that of the *amarylla* and *colorada*; but grows in a cold froſty climate: By which means the bark is not only alter'd in its quality, but is alſo whitiſh on the outſide, tho' cinnamon-colour'd within ſide, and in medicines ought to be rejected. This ſort and the *blanca* grow plentifully in the province of *Ayavaca*, 50 leagues from *Piura*, and 62 from *Payta*, a port in the ſouth-ſea; as alſo in *Cariamango*, *Gonſonama* and *Ximburo*, whence they commonly ſend it to *Payta*, and there ſell it as the beſt ſort. The *blanca* likewiſe grows in the province of *Quenca*, and in the mountains of *Caxamarca*: But the true and genuine fine *Jeſuits* bark, which is of a reddiſh or yellowiſh colour, is only found from 5 to about 14 leagues round the city of *Loxa*, in the province of *Loxa*, which the *Spaniards* generally call *provincia de las calvas*. This city is ſituated between two rivers, that run into the great river *Marannon*, or the river of the *Amazons*, and lies about 100 leagues from *Payta*, and in a direct line about 110 leagues ſouth-eaſt from *Guayaquil*, tho' by the common road near 200. The places about *Loxa*, where this fine ſort is found, are, *La Sierra de Caxanuma Malacaios*, *Yrutaſinga*, *Yangana*, *Manſanamace*, *La ſierra de Boqueron*, and a place call'd *Las Monſas*.

The bark-trees do not grow altogether in one ſpot, but intermixt here and there with ſeveral others in the woods: It happens, ſometimes indeed, that cluſters of them are found together, tho' at preſent they are much ſcarcer than in former times; a great many of the fine large bark trees having been entirely cut down, that their bark might be the more eaſily ſliced off.

The ſoil, where the beſt ſort thrives, is generally in red clayey or rocky ground, and very frequently on the banks of ſmall rivers deſcending from high mountains.

That this tree flouriſhes and bears fruit at the ſame time all the year round, is certainly owing to the almoſt uninterrupted rains that fall in thoſe high mountains where it grows, which continue with little or no intermiſſion: Tho' about 3 or 4 leagues down in the low country, where it is exceſſively hot, there are wet and dry ſeaſons, as in other hot countries; the rains beginning in *December* and ending in *May*: This ſeaſon, the *Spaniards*, who live there, call *temporal*, and it is general all thereabouts; whereas what they call *paroma* is a cold rainy ſeaſon, that laſts in all the mountainous places of theſe countries from *June* to *November*, but eſpecially in the city of *Loxa* and places adjoining, where Mr. *Arrot* had paſs'd 25 or 30 days without ſeeing the ſun once; and felt the air ſo exceeding cold, that he was obliged to be always wrapped up in his cloak, and to be in continual motion to keep himſelf warm. Such exceſſive cold ſo near the line appears to *Europeans* incredible; but many places in theſe latitudes are ſo, by their ſituation and vicinity to high mountains.

The propereſt ſeaſon for cutting the bark is from *September* to *November*, the only time in the whole year that there is ſome intermiſſion from rain in the mountains. Having diſcover'd a ſpot where the trees moſt abound, they firſt build huts for the workmen, and then a large hut in which to put the bark, in order to preſerve it from the wet: But they let it lie there as ſhort a time as poſſible, having before-hand cut a road from the place where the trees grow, thro' the woods, ſometimes 3 or 4 leagues, to the neareſt plantation or farm-houſe in the low country, whither, if the rain permit them, they carry the bark forthwith to dry. Theſe preparations made, they provide each *Indian* (they being the cutters) with a large knife, and a bag that can hold about 50 pounds of green bark: Every two *Indians* take one tree, whence they cut or ſlice down the bark, as far as they can reach from the ground; they then take ſticks about half a yard long each, which they tie to the tree with tough withs at proper diſtances like the ſteps of a ladder, always ſlicing off the bark, as far as they can reach, before they fix a new ſtep, and thus mount to the top, the *Indian* below gathering what the other cuts; this they do by turns, and go from tree to tree, till their bag is full; which, when they have plenty of trees, is generally a day's work for one *Indian*. As much care as poſſible muſt be taken that the bark be not cut wet; ſhould it ſo happen, it muſt be carried directly down to the low country to dry: For, otherwiſe it loſes its colour, turns black

black and rots; and if it lie any time in the hut without being spread abroad, it runs the same risque: So that while the *Indians* are cutting, the muscles (if the weather permits) ought to be carrying it down to the place appointed for drying it; which is done by spreading it abroad in the open air, and frequently turning it.

Mr. *Arrot* had the curiosity to send upwards of 50 seroons from the woods to the city of *Loxa*, where he put it into a large open house, and dried it under cover, never exposing it either to the sun or night air, imagining that the sun exhal'd a great many of its fine parts; and that the night air, or *serene*, was very noxious to it: But he found the colour of the bark, thus cur'd, not near so bright and lively as that dried in the open air. He is of opinion that a very short time will put an end to this best sort, or at least, that it will be extremely hard to be got, by reason of its distance from any inhabited place, the impenetrability of the woods where it grows, and the scarcity of the *Indians* to cut it, who, by the *Spaniards* hard usage and cruelty, are daily diminishing so fast, that in a very few years their race in that country shall be quite extinct.

Mr. *Arrot* says, that the small bark which curls up like sticks of cinnamon (and which in *England* is much valued, as being cut off the branches; and therefore reckon'd better and more effectual in curing fevers) is only the bark of the younger trees; which, as it is very thin, curls in that manner; and that the bark of the branches would not compensate the trouble and expences of cutting. He likewise affirms, that after the bark is cut off any tree it requires at least 18 or 20 years to grow again; which is directly contrary to what Dr. *Oliver* says in *Phil. Trans.* N° 290. He added besides, that its fruit is no ways like a chesnut, as the Dr. informs us in the same *Transaction*; but rather like a pod, which incloses a seed somewhat like a hop-seed; and that he had sent some of them to *England*.

He could not tell by what artifice or stratagem the *Jesuits* had got this bark to be call'd after them, if not that they carried it first into *Europe*, and gave themselves out as the first discoverers of its virtues: But Mr. *Arrot* assured, that the current opinion at *Loxa* is, that its qualities and use were known to the *Indians* before ever any *Spaniard* came among them; and that it was by them applied in the cure of intermitting fevers, which are frequent over all that wet unhealthy country.

Observation of the Moon's *transit by* Aldebaran, *at* London April 3, 1736; *by Dr.* Bevis. Phil. Tranſ. N° 446. p. 90.

Fig. 17. Plate IX. the glaſs inverting the objects.

Apparent time. H.	M.	S.	
7	40	0	The moon's body and *Aldebaran* ſeen together in the diſtinct baſe of the teleſcope.
7	45	52	The moon's ſouthern limb running along the parallel thread; the weſtern limb came to the horary thread.
7	49	41	The glaſs remaining fixt, and *Aldebaran* running along the parallel thread (having the ſame declination with the moon's ſouthern limb) came to the interſection of the threads.
8	13	4	The moon again running along the parallel came to the horary thread.
8	15	50	*Aldebaran* (the teleſcope remaining fixt) came to the firſt oblique thread at *c*.
8	15	54 ½	— to the horary thread at *b*.
8	15	59	— to the ſecond oblique thread at *a*.
8	59	54	*Aldebaran* in the line paſſing thro' the cuſps, his neareſt diſtance from the moon's body being ſomewhat leſs than the length of *Mare Criſium*, or $\frac{1}{10}$ of the moon's diameter nearly. Fig. 18. repreſents the viſible way of the moon.

A lunar Eclipſe *obſerv'd in* Fleet-ſtreet, London, Sept. 8, 1736; *by Mr.* George Graham, *and Mr.* James Short *of* Edinburgh. Phil. Tranſ. N° 446. p. 92.

September 8, 1736	Apparent time. H.	M.	S.
Beginning of the eclipſe	12	58	0
The ſhadow touch'd *Grimaldi*	13	0	0
—— *Kepler*		9	30
—— *Copernicus*		17	10
—— the eaſt ſide of *Tycho*		25	5
—— the eaſt ſide of *Plato*		34	30
—— the eaſt ſide of *Manilius*		36	40
—— the eaſt ſide of *Mare Criſium*		56	20
Beginning of total darkneſs	14	3	45

The

The obſervation was made with a 5 ½ inches reflecting teleſcope, magnifying about 38 times.

A total Eclipſe of the Moon *obſerv'd* Sept. 8, 1736, O. S. *in* Covent-Garden, London; *by Dr.* Bevis. Phil. Tranſ. N° 446. p. 93. *Tranſlated from the* Latin.

The obſervation was made with a 5 foot teleſcope.

Apparent time H.	M.	S.	
12	53	25	The *penumbra* tinges the north-eaſt limb of the moon: The ſky ſerene.
	54	25	The *penumbra* now pretty ſenſible: Serene.
	56	50	The true ſhadow, as far as the Dr. could judge, touches the moon's limb: Serene.
	57	30	The ſhadow touches *Grimaldi*; ſerene.
13	00	25	The ſhadow covers *Grimaldi*: The ſky pretty ſerene.
	7	23	It enters *Mare humorum*; obſerv'd thro' thin clouds: Afterwards the clouds became very thick.
	28	39	The ſhadow touches *Mare vaporum*: Serene.
	31	19	The darken'd part of the moon is ſeen of a reddiſh colour, as it were: The ſky exceeding ſerene.
	36	53	The limit of the ſhadow biſſects *Manilius*, and touches *Mare ſerenitatis*. Exceeding ſerene.
	38	48	It touches *Mare tranquillitatis*: Serene.
	47	21	*Mare ſerenitatis* cover'd: Serene.
	55	26	The ſhadow touches *Mare Criſium*: Serene.
	58	5	*Mare fœcunditatis* cover'd: Serene.
14	2	25	The total immerſion of the moon. Very thick clouds come on; and the moon is no longer ſeen, till
16	43	0	*Mare tranquillitatis*, as it appears, entirely emerged — thro' the breaks of the clouds.
	43	30	Clouds again.
17	3	22	The cloud going off, the moon ſeem'd entirely clear.

The clock was adjuſted to true time by equal altitudes of the ſun, and its agreeing with Mr. *Graham*'s was mark'd, by means of a very good pocket-watch.

A

A total Eclipſe *of the* Moon *obſerved at* Wittemberg *in* Saxony, September 8, 1736, O. S. *in the Morning*; *by M.* Weidler. Phil. Tranſ. N° 446. p. 94. *Tranſlated from the* Latin.

H.	M.	S.	
1	36	0	The *penumbra* comes on the eaſtern part of the moon like a ſmoke or fog.
1	50	0	The beginning.
1	50	30	The ſhadow comes to *Grimaldi.*
1	52	0	——— touches *Galilæus.*
2	0	0	——— *Kepler.*
2	1	30	*Kepler* entirely cover'd.
2	7	0	The portion of the moon's diſk, that is deeper immerſed in the ſhadow, appears brighter than that, nearer the edge of the ſhadow.
2	8	0	The ſhadow comes to *Copernicus.*
	10	50	—— entirely covers *Copernicus.*
	16	10	The ſhadow comes to *Tycho.*
	20	0	Half the moon obſcur'd.
	25	0	The ſhadow comes to *Mare ſerenitatis.*
	29	10	——— to *Menelaus.*
	36	0	*Mare ſerenitatis* entirely cover'd.
			At this time the moon appears red thro' the ſhadow like a live-coal.
	45	30	The ſhadow comes to *Mare Criſium.*
			At this time the edge of the ſhadow about *Mare Criſium* is inwardly incurvated.
			And all the time of the eclipſe the periphery of the ſhadow is rough and diſtinguiſhed by ſeveral prominences; and in the extremity it is ſeen encompaſſed as with a thin ſmoke.
	50	0	*Mare Criſium* entirely cover'd.
	53	0	Total obſcuration.
			At this time about a third part of the moon's diſk towards the eaſt appears darker than the weſtern part.
3	43	0	The ſhadow appears denſer in the middle, and more dilute towards the extremities.
4	8	0	The moon is cover'd with clouds.
4	44	0	The emerſion of the moon out of the ſhadow.
4	45	0	The ſhadow goes off *Grimaldi.*

Clouds

Clouds afterwards hid the moon; and tho' ſhe emerged again out of them, yet a fog or rarer cloud darkens her in ſuch manner, that the *maculæ* could not be diſtinguiſh'd: At length the moon is entirely hid by denſer clouds.

The obſervation was made with a teleſcope of 8 *Paris* feet.

A lunar Eclipſe *obſerv'd* Sept. 8, 1736, *in* Hudſon's-bay; *by Capt.* Chriſtopher Middleton. Phil. Tranſ. N° 446. p. 96.

CAPT. *Middleton*, being in *Hudſon's-bay* (in Lat. 55 degrees, 34 minutes, north, and on the meridian of the *north-bear-iſland*, which lies 30 miles to the weſtward of *Charlton*) obſerved a total eclipſe of the moon on *Sept.* 8, 1736: The weather was very clear; but the motion of the ſea render'd his teleſcope uſeleſs, and he miſs'd the beginning.

	H.	M.	
The total immerſion of the moon's body into the ſhadow. — —	8	22	by his watch
The emerſion — — —	10	8	by ditto
The end — — —	11	16	by ditto

In order to rectify his watch, and be ſure of the true time, he took 3 ſeveral altitudes next morning, and one in the afternoon, by Mr. *Hadley*'s and Mr. *Smith*'s quadrants; which (having made proper allowances for the refraction of the atmoſphere and the height he ſtood above the ſurface of the ſea) were, as follows,

	Deg.	Min.		H.	M.	
Firſt altitude	23	0	hence the true time	8	49	—
Latitude	55	45	the time by the watch	8	28	
			Watch too ſlow	0	21	—

Deg.

	Deg.	Min.		H.	Min.
Second altitude	25	48	The true time therefore, is	9	15 —
Latitude	55	45	The time by the watch	8	54
			Watch too slow	0	21 —
Third altitude	26	44	The true time, therefore, is	9	24
Latitude	55	45	The time by the watch	9	3
			Watch too slow	0	21 —
The fourth altitude taken the same day in the afternoon	21	29	Hence the true time is	3	25 +
Latitude	55	33	The time by the watch	3	04
			Watch too slow	0	21 +

If, therefore, 21 minutes be added to the times above-mention'd for the error of the watch, we shall have the true times of the several observations on the meridian of the *north-bear-island*, as follows, *viz.*

	H.	M.
The total immersion of the moon's body into the shadow — — — —	8	43
The emersion — — — —	10	29
The end — — — —	11	37

This same eclipse was observ'd *Sept.* 8, 1736. by Dr. *Bevis* at *London*; and he made the true time of the total immersion of the moon's body into the shadow, 14 hours, 2 minutes, 25 seconds; consequently, the difference of longitude between *London* and *North bear-island* in *Hudson's bay* is 5 hours, 19 minutes, 25 seconds, or 79 degrees, 51 minutes.

l solar eclipse observed at London, Sept. 23, 1736; *By* Dr. Bevis. Phil. Trans. N°. 446. p. 98. *Translated from the* Latin.

pp. time, P. M.
. M. S.

12 35 The northern limb of the sun (Fig. 19. Plate IX.) running along the parallel thread P. P. his western limb touches the horary thread H. H.

12 42 The little macula near the northern limb comes to the first oblique thread 1.

13 1 The macula at the horary thread H. H.

13 20 The macula at the second oblique thread 2.

14 45 The eastern limb of the sun at the horary thread: afterwards clouds.

45 41 The sun emerging out of the clouds; the eclipse, it is true, is begun by the telescope, but now scarcely seen begun.

45 48 It is still imperceptible to the naked eye through a colour'd glass.

46 0 Now it is pretty sensible. Then clouds.

5 29 The southern limb (Fig. 20.) running along the parallel, the western limb at the horary thread.

5 41 The western cusp of the sun at the horary thread.

7 5 The eastern cusp at the horary thread.

7 39 The eastern limb at the horary thread.

Then the sun is overcast with clouds till its setting.

The Dr. puts the beginning at 4 h. 45 m. 31 sec. p. m.

An occultation of Mars *by the* Moon, *observed in* Fleet-street, London, Oct. 7, 1736, *by Mr.* George Graham, F. R. S. Transl. N°. 446. p. 100.

THE observation was made with a refracting telescope of 12 foot.

Apparent time.
H. M. S.

At 14 24 44 *Mars* appeared above half covered; but a distinct view could not be had for flying clouds.

14 25 21 *Mars* entirely covered, the last ray of light being then lost.

Apparent time.
H. M. S.
15 11 22 The moon appeared, but *Mars* was not ſeen, no part being yet emerged.
15 15 11 He judged it was quite emerged; but clouds hindered the moon's limb from being diſtinctly ſeen.

The ſame obſerved in Covent-Garden, London, Oct. 7, 1736. *by Dr.* Bevis. Phil. Tranſ. N°. 446. p. 101.

BEFORE the eclipſe, Dr. *Bevis* took ſeveral differences of right aſcenſion and declination between *Mars* and μ of *Piſces*, for aſcertaining the true place of that planet.

App. time, P. M.
H. M. S.
14 24 10 The Dr. was ſurprized to ſee *Mars* continue quite round, tho' hardly, to appearance, disjoined from the ſcabrous edge of the moon: But that inſtant he thought it began to loſe its figure. — Clouds.
14 25 26 The moon ſhone out bright again; but *Mars* entirely vaniſhed.
15 14 46 The moon being juſt clear of a cloud, he ſaw *Mars* partly emerged.
15 14 49 *Mars* ſeemed juſt half out: Then clouds again; ſo that he did not ſee the final contact.

The moon's diameter was 21,157 parts of the micrometer; and its illuminated part paſſed over the horary thread in 2 m. 3 ſec.

The Dr. is certain of the time to 2 or 3 ſec.

A Tranſit of Mercury *over the* Sun, *obſerved in* Fleet-ſtreet, London, Oct. 31, 1736. *by Mr.* George Graham. Phil. Tranſ. N°. 446. p. 102.

App. time, A. M.
H. M. S.
At 9 22 0 *Mercury* not yet ſeen; then clouds.
25 37 Mr. *Graham* firſt ſaw *Mercury* for a few ſeconds; and judged he was got intirely within the ſun's diſk, or perhaps a little more: Then clouds again, with ſome intervals of a few moments between, which allowed a ſight of *Mercury* about three or four ſeveral times; then

App. time, A. M. H. M. S.	
	then quite cloudy till near 12, when he had a ſight of the ſun for a few minutes, and took his tranſit upon the meridian; at which time *Mercury* was judged to be about two of his diameters, or a little more, within the ſun's diſk, and a little paſt the vertical line.
12 10 27	He had again a ſight of the ſun, but *Mercury* was gone off.

A Tranſit of Mercury *over the* Sun, *at the Obſervatory of* Bononia, Nov. 9, 1736, N. S. *in the morning*; *by* S. Manfredi. Phil. Tranſ. N°. 446. p. 103. *Tranſlated from the* Latin.

IN the higheſt room of the obſervatory, S. *Zanotti* directed towards the ſun a very good teleſcope of *Campani*'s, of 22 *Bononian* feet in length, to obſerve, if poſſible, the very ingreſs of *Mercury* on the ſun's limb; and others from other parts of the obſervatory directed ſhorter teleſcopes for the ſame purpoſe. The ſky was very clear; and there was not a breath of wind ſtirring. S. *Roverſius* had the good fortune firſt of all to diſcover the planet at the ſun's limb at 22^h 8′ 37″ p. m. and immediately determine his interior contact with the ſun at 22^h 11′ 12″. The clocks were adjuſted for the ſame days to the meridian line, which *Zanotti* had ſeveral times compared with the ſun by equal altitudes morning and evening.

Other obſervers found the planet ſomewhat later on the ſun's limb; and S. *Manfredi*, directing from a lower room an 11 foot teleſcope of *Campani*'s, did not ſee the planet before 22^h 9′ 5″; when now it touched the ſun with a pretty conſiderable part of its body; but its interior contact by the ſame teleſcope was at 22^h 10′ 53″. But the former obſervation is much more certain, as having been taken with a better inſtrument; yet ſince it appeared from the times of the planet's going off, as ſhall be mentioned anon, that it ſpent therein 3′ 16″, if we ſubſtract ſo much from the time of interior contact obſerved by *Roverſius*, the exterior contact, or the firſt appulſe of *Mercury* to the ſun will be ſtill more certain, namely 22^h 7′ 56″:

Afterwards the obſervations were made with a view to find ſome points of the viſible path of the planet on the ſun's diſk: All theſe points were referred to the horary circle, as alſo to the parallel drawn thro' the ſun's centre, according to *Caſſini*'s Me-

th d, marking the times by a clock, in which the ſun's limbs and *Mercury* paſſed the horary thread of the micrometer: And the latter beſides paſſed the oblique threads, whilſt the ſun with its northern limb graſed along the parallel thread itſelf. With an eight foot teleſcope *Zanotti* found a great many ſuch points; and S. *Manfredi* one or two, with a ſix foot teleſcope, fitted with an exquiſite micrometer, invented by the celebrated S. *Manfredonius*, *Ceſarean*-Mathematician, and preſented by him to this Obſervatory. With the ſame teleſcope both S. *Roverſius* and Dr. *Perelli* determined ſome other points. And to this likewiſe is referable the obſervation made by the latter in the meridian, with a mural ſemicircle, whereby the right aſcenſion of the planet was found greater by 11 ſeconds of time and $\frac{1}{2}$; and its declination leſs by 58 ſeconds and $\frac{1}{2}$ than that of the ſun's centre. Beſides, *Zanotti* took upon himſelf to deſcribe the poſitions of the more remarkable maculæ, which were ſeen in great numbers that day on the ſun. It was eaſy to diſtinguiſh the planet from theſe maculæ, both as it was exactly round, and very black, and ſurrounded with no *areola*.

As to *Mercury*'s egreſs, S. *Algarotti*, who came to *Bononia* in order to view this phænomenon, obſerved with an eight foot teleſcope the beginning at 50 min. 1 ſec. p. m. the end at 53 min. 6 ſec. But S. *Manfredi* with the 11 foot teleſcope above mentioned obſerved the beginning at 51 min. 7 ſec. and the end at 53 min. 44 ſec. *Roverſius* with a 14 foot teleſcope only obſerved the end at 54 min. 1 ſec. But theſe obſervations are leſs certain, both as the teleſcopes were but indifferent, and as a wind ariſing at that time diſturbed them a little. Therefore, the obſervation with the 22 foot teleſcope, whereby S. *Franciſcus Vandelius* determined the interior contact at 50′ 50″ and the exterior at 54′ 6″, is to be preferred to all the others: Whence the *mora* of the planet on the ſun's limb was 3′ 16″, and the time of the egreſs of the centre 52′ 28″, which according to S. *Manfredi*'s obſervation ſhould be 52′ 25″.

Thus far the obſervations themſelves: Now S. *Manfredi* comes to what may be deduced from comparing them together. Aſſuming the ſun's diameter 32 min. 34 ſec. and the time of his tranſit over the horary circles 2 min. 17 ſec. (which numbers are exhibited in the tables of modern aſtronomers, and verified by the obſervations themſelves) theſe points of the planetary path defined by obſervation are repreſented Fig. 21. Plate IX. And ſince by reaſon of the ſmall errors in the obſervations all the points did not exactly fall

fall into the same right line, there was no fitter way of reconciling them than by supposing a perpendicular drawn from the centre of the sun to the path of the planet to form an angle with the horary circle of 23 degrees, 40 min. to the east; and put the length of that perpendicular from the centre to the path itself 13 min. 58″ to the north. From these all the rest was calculated as follows:

	H.	′	″
The beginning of mercury's ingress upon the sun's disk	22	7	56
The ingress of the centre	22	9	34
The total ingress	22	11	12
The beginning of the egress	0	50	50
The egress of the centre	0	52	28
The total egress	0	54	6
The *Mora* of mercury's centre on the sun's disk	2	42	54
The *Semi-mora*	1	21	27
The time of the mean transit	23	31	1

	Deg.	Min.	Sec
The angle of the perpendicular to the path of the planet with the horary circle, determin'd by the observations to the east.	23	40	0
The angle of the eclip. with the horary circle from astronomical tables to the east	105	48	0
Thence the angle of the ecliptic with the perpendicular to the apparent path of mercury	82	8	8
And the angle of the apparent path with the ecliptic	7	52	0
The distance of the path from the centre of the sun found by the observations to the north	0	13	58
The sun's semi-diameter	0	16	17
The length of the path within the sun's disk	0	16	45
The half of that length	0	8	22

From

	H.	′	″
From these the horary motion of mercury in the apparent path	0	6	10
The apparent horary motion in the ecliptic	0	6	6
Hence the portion of the path between the middle of the transit and the conjunction	0	1	58
The portion of the path from the ingress till the conjunction	0	10	20
The portion of the same from the conjunction till the egress	0	6	24
The difference of the longitude of mercury and the sun in the ingress	0	10	15
The difference of longitude in the egress	0	6	21

	H.	′	″
The time from the middle of the transit till the conjunction	0	19	2
The very time of the conjunction at *Bononia* true time	23	50	3
mean time	23	34	25
The longitude of the sun and mercury in the very conjunction according to *Cassini*'s tables	19 of scorpio	23	30

With this longitude corresponds within four seconds an observation made the same day by S. *Peter Lilius* with a meridian gnomon at S. *Petronio*'s church.

	°	′	″
The latitude of mercury in the ingress north	0	12	37
Lat. in the egress north.	0	14	54
Thence the horary motion in lat.	0	0	50½
And the lat. in the very conjunction north	0	14	1

	H.	′	″
Hence the interval of time from the transit of mercury thro' the ascending node till the conjunction	16	39	0
The very time of the transit thro' the node true time	7	11	0
mean time	6	55	0

As-

	Deg.	′	″
According to *Caſſini*'s tables, the motion of mercury in his orbit, ſeen from the ſun in the ſpace of 16^h 39′ about this time, or the argument of latitude in the conjunction	4	15	47
The ſame motion reduced to the ecliptic	4	13	56
Thence the place of the aſcending node of mercury ſeen from the ſun	15	9	34
	of Taurus		
The diſtance of mercury from the ſun at the time of conjunction according to *Caſſini*'s tables	Log. 449301		
The diſtance of the earth from the ſun according to the ſame tables	Log. 499503		

	Deg.	′	″
Hence the N. latitude of mercury at the time of conjunction, ſeen from the ſun	0	30	31
	°	′	″
Whence the inclination of mercury's orbit to the ecliptic	6	51	0

	H	′	″
The time from the interior to the exterior contact of mercury at his going off, by obſervation	0	3	16
The portion of the path run over by mercury in this time.	0	0	20
	°	′	″
The angle of the path with the ſun's ſemi-diameter in the egreſs	58	50	0
Thence the apparent diameter of mercury very nearly.	0	0	10

The tranſit of Mercury *over the* Sun, Nov. 11. 1736. N. S. *obſerved by* M. Weidler. Phil. Tranſ. N° 446. p. 110.

THO' the uncertain ſtate of the weather was ſome hindrance to a compleat obſervation of the tranſit of mercury over the ſun, *Nov.* 11. 1736. N. S. Yet what M. *Weidler* obſerv'd, he thought proper not to deprive others of; chiefly becauſe it did not appear to him, that this phenomenon had hitherto been obſerv'd with better ſucceſs.

Mercury appeared within the ſun's eaſtern limb, as repreſented, Fig. 22. Plate IX.

H.	M.	S.		
10	49	20	at	1
11	36	0	mercury about	2
11	52	20	at	3
12	2	30		4
12	4	30		5
	44	20		6
	52	45		7

Obſervations made at Oxford *on the Comet that appeared in* January, February, *and* March, 1737; *by Mr.* James Bradley. Phil. Tranſ. N° 446. p. 111.

MR. *Bradley* made ſeveral obſervations on the late Comet, during the laſt five weeks of its appearance, which enabled him to find out the elements of a parabolic trajectory, upon which a calculus might be founded that would correſpond with each of his obſervations within about a minute of a degree: But the firſt of them being taken many days after the time of the *perihelion*, and the whole ſeries comprehending but a ſmall portion of the trajectory; he was ſenſible that a little error, either in the obſervations themſelves, or in the places of the fixt ſtars, with which the comet was compar'd, might occaſion a conſiderable difference in the ſituation and magnitude, *&c.* of the orbit, deduced from them alone: And therefore he was deſirous of having ſome earlier and accurate obſervations, in order to determine thoſe elements with more certainty: But not having hitherto been able to procure ſuch, he now communicates the particulars of his own, together with the compariſon between the obſerved places of the comet, and thoſe computed from ſuch elements as he has already collected from his own obſervations.

He firſt ſaw the comet on the 15th of *February*, 1737, between ſix and ſeven in the evening, when its nucleus appear'd ſmall and indiſtinct; and its tail (extending above a degree from the body) pointed towards the ſtar *in lino auſtral.* of *Piſces*, mark'd ξ by *Bayer*. Upon applying his micrometer to a good ſeven foot teleſcope, he obſerv'd, that at ſeven hours, 32 min. equat. time, the comet preceded the ſaid ſtar one degree, one minute, 40 ſeconds in right aſcenſion, and was 26 minutes, 20 ſeconds, more ſoutherly than the ſtar.

Note, That the equal time is likewiſe made uſe of in all the following obſervations.

Aſſu-

Assuming the place of this star, as settled in the *British Catal.* (as Mr. *Bradley* likewise does the places of others hereafter mention'd) it follows, that the comet's right ascension was 23 degrees, 58 minutes; and its declination 1 deg. 31 min. 55 seconds north.

February 17. 7 hours, 33 min. the comet follow'd *α in nodo lini Piscium* 31 min. 25 seconds in *Right Ascension*; and was 52 min. 30 seconds more northerly: Hence the comet's *Right Ascension* was 27 deg. 38 min. 20 seconds; and its declination 2 deg. 21 min. 10 sec. north.

February 18. 7 hours 14 min. a small star (whose *Right Ascension* was afterwards found to be 29 deg. 5 seconds; and declination 2 deg. 58 min. 30 sec. north) preceded the comet 24 min. in *Right Ascension*; and was 15 min. 30 sec. more northerly. Hence the comet's *Right Ascension* was 29 deg. 24 min. 5 sec. and its declination 2 deg. 34 min. north.

February 21. 7 hours, 25 min. the comet preceded *ν Ceti* 1 deg. 6 min. in *Right Ascension*; and was 38 min. 20 sec. more southerly. Hence the comet's *Right Ascension* was 34 deg. 25 min. 10 sec. and its declination 3 deg. 47 min. 20 sec. north.

Feb. 22. 7 hours, 45 min. the comet follow'd *ν Ceti* 30 min. 5 sec. in *Right Ascension*; and was 18 min. 45 sec. more southerly: Hence the comet's *Right Ascension* was 36 deg. 1 min. 15 sec. and its declination 4 deg. 6 min. 55 sec. north.

Feb. 25. 7 hours 45 min. a small star (whose *Right Ascension* was afterwards found to be 40 deg. 34 min. and declination 5 deg. 5 min. 30 sec. north) follow'd the comet 2 min. 30 sec. in *Right Ascension*; and was 2 min. 30 sec. more northerly than the comet: Hence the comet's *Right Ascension* was 40 deg. 31 min. 30 sec. and its declination 5 deg. 3 min. north.

The difference of *Right Ascension* and declination between this star and the comet was taken with a fifteen foot telescope; but the place of the star was determin'd by one observation made with the 7 foot telescope.

Feb. 27. 8 hours 45 min. the comet preceded a small star 1 deg. 16 min. in *Right Ascension*; and was 2 min. 15 sec. more southerly. The *Right Ascension* of this star was afterwards (by a single observation) found to be 44 deg. 37 min. 40 sec. and its declination 5 deg. 38 min. 30 sec. north: Hence the comet's *Right Ascension* 43 deg. 21 min. 40 sec. and its declination 5 deg. 36 min. 15 sec. north.

Mar. 4. 8 hours a ſmall ſtar (whoſe *Right Aſcenſion* was found to be 49 deg. 30 min. 30 ſec. and its declination 6 deg. 38 min. 30 ſec. north) preceded the comet 7 min. 30 ſec. in *Right Aſcenſion*; and was 10 min. more ſoutherly: Hence the *Right Aſcenſion* of the comet was 49 deg. 38 min. and its declination 6 deg. 48 min. 30 ſec.

Mar. 12. 8 hours 25 min. the comet preceded μ of *Taurus* 2 deg. 5 min. 50 ſec. in *Right Aſcenſion*; and was 4 min. 25 ſec. more northerly than the ſtar: Hence the comet's *Right Aſcenſion* was 58 deg. 12 min. 40 ſec. and its declination 8 deg. 16 min. 50 ſec. north.

Mar. 14. 9 hours the comet follow'd the 47th ſtar of *Taurus* in *Brit. Catal.* 12 min. 50 ſec. in *Right Aſcenſion* and was 15 ſec. more northerly than the ſtar: Hence the comet's *Right Aſcenſion* was 60 deg. 8 min. 5 ſec. and its declination 8 deg. 34 min. 5 ſec. north. This and all the following obſervations were made with a good 15 foot teleſcope; the comet now appearing too faint to be well obſerv'd with the 7 foot teleſcope.

Mar. 17. 8 hours 40 min. the comet follow'd γ of *Taurus* 25 min. 5. ſec. in *Right Aſcenſion*; and was 9 min. 40 ſec. more northerly: Hence its *Right Aſcenſion* was 62 deg. 47 min. 55 ſec. and its declination 8 deg. 58 min. 45 ſec. north.

Mar. 19. 7 hours 50 min. the comet follow'd the ſame ſtar 2 deg. 4 min. 50. ſec. in *Right Aſcenſion*, being 23 min. 55 ſec. more northerly: Hence its *Right Aſcenſion* was 64 deg. 27 min. 40 ſec. and declination 9 deg. 13 min. north.

The ſame night at 9 hours the comet preceded *d* of *Taurus* 47 min. 40 ſec. in *Right Aſcenſion*; and was 22 min. 50 ſec. more ſoutherly: Hence its *Right Aſcenſion* was 64 deg. 30 min. 20 ſec. and declination 9 deg. 12 min. 35 ſec. north.

Mar. 20. 8 hours 5 min. the comet preceded *d* of *Taurus* 30 ſec. in *Right Aſcenſion*; and was 16 min. 35 ſec. more ſoutherly than the ſtar: Hence its *Right Aſcenſion* was 65 deg. 17 min. 30 ſec. and declination 9 deg. 18 min. 50 ſec. north.

Mar. 22. 8 hours 15 min. the comet follow'd the ſame ſtar 1 deg. 36 min. 10 ſec. in *Right Aſcenſion*; and was 3 min. 50 ſec. more ſoutherly: Hence its *Right Aſcenſion* was 66 deg. 54 min. 10 ſec. and declination 9 deg. 31 min. 35 ſec. north.

This was the laſt night Mr. *Bradley* ſaw the comet: For, the moon, being then in her increaſe, entirely obſtructed its farther appearance. The light of the comet was indeed (even in the moon's abſence) ſo very weak, that he found it difficult in ſome of the latter obſervations, to take its place with any tolerable certainty;

certainty; which is, in part, the cauſe of ſome little diſagreement obſervable in the comet's places, taken from the ſame ſtars on different nights; tho' there are likewiſe other irregularities that occur in this ſeries of obſervations, which ſeem to ariſe from ſmall errors in the aſſumed places of the fixt ſtars.

Suppoſing the trajectory deſcrib'd by this comet to be nearly parabolical, conformable to what Sir *Iſaac Newton* has deliver'd in the third book of his *Princip. Mathem.* Mr. *Bradley* gathers from the foregoing obſervations, that the motion of this comet in its own orbit was direct; and that it was in its perihelion *Jan.* 19. 8 hours 20 min. equat. time at *London.* That the inclination of the plane of the trajectory to the ecliptic was 18 deg. 20 min. 45 ſec. The place of the deſcending node 16 deg. 22 min. of *Taurus.* The place of the perihelion 25 deg. 55 min. of *Aquarius.* The diſtance of the perihelion from the deſcending node 80 deg. 27 min. The logarithm of the perihelion diſtance from the ſun 9.347960. The logarithm of the diurnal motion 0.938188.

From theſe elements (by the help of Dr. *Halley*'s general table for comets, to which they are adapted) Mr. *Bradley* computed the places in the following table; which alſo contains the longitudes and latitudes of the comet, calculated from the obſerv'd *Right Aſcenſions* and declinations above-mention'd, together with the differences between the obſerved and computed places.

equat. time at *Oxford.*			observed long. of the comet.			South. Lat. observ'd.		
	D.	H.	M.	D.	M.	S.	D.	M.	S.
Feb.	15	7	32	♈ 22	45	7	7	53	27
	17	7	33	26	30	30	8	27	21
	18	7	14	28	18	14	8	44	20
	21	7	25	♉ 3	26	34	9	26	50
	22	7	45	5	4	53	9	40	00
	25	7	45	9	42	18	10	12	21
	27	8	45	12	36	43	10	31	42
Mar.	4	8	00	19	3	00	11	6	46
	12	8	25	27	49	58	11	43	3
	14	9	00	29	47	42	11	49	59
	17	8	40	♊ 2	30	57	11	56	31
	19	7	50	4	12	36	12	00	19
		9	00	4	15	11	12	1	12
	20	8	5	5	3	10	12	3	5
	22	8	15	6	41	30	12	6	15

computed Long. of the comet.			S. Lat. computed.			Diff. of Long.		Diff. of Lat.	
D.	M.	S.	D.	M.	S.		S.		S.
♈ 22	45	00	7	53	1	+	7	+	26
26	30	44	8	28	6	−	14	−	45
28	17	46	8	43	57	+	28	+	23
♉ 3	26	53	9	26	46	−	19	+	4
5	5	28	9	39	27	−	35	+	33
9	41	19	10	12	22	+	59	−	1
12	36	16	10	31	13	+	27	+	29
19	3	5	11	7	8	−	5	−	22
27	49	53	11	43	19	+	5	−	16
29	47	19	11	49	26	+	23	+	33
♊ 2	30	50	11	56	49	+	7	−	18
4	12	45	12	00	47	−	9	−	28
4	15	13	12	00	52	−	2	+	20
5	3	32	12	2	33	−	22	+	32
6	41	19	12	5	42	+	11	+	33

From the ſmall differences between the comet's obſerv'd and computed places, exhibited in the two laſt columns of this table,

ble, we may reaſonably conclude, that the orbit, as above determin'd, cannot differ much from the truth, and muſt therefore be near enough to enable future aſtronomers to diſtinguiſh this comet upon another return, and thereby ſettle its period; which Mr. *Bradley* could not then pretend to do, not having met with an account of any former comet that ſeemed likely to have been the ſame with this, of which a deſcription has been given particular enough to determine this point.

A Comet *obſerved in* February, 1737, *from the* Aventine-hill *at* Rome; *by the* Abbè de Revillas. Phil. Tranſ. N° 446. p. 118. *Tranſlated from the* Latin.

ON the 16. of *February*, 1737, about 7 o'clock *p. m.* the comet appear'd for the firſt time in the weſtern part of the heavens, 8 or 9 degrees lower than *Venus*, and declining a little towards the ſouth from her vertical circle. With the naked eye nothing was obſerved but a ſmall whitiſh line, that ſhone with a faint light; yet with a very good teleſcope of *Campani*'s, of 6 foot, beſides the tail, which extended to the oppoſite part from the ſun, and appear'd like a ſmall line without the teleſcope, was likewiſe eſpied the *nucleus*, tho' encompaſſed all round with a thin atmoſphere. As there was then no quadrant at hand, and not only a fog intercepted, but the twilight deprived the view of the neighbouring fixt ſtars, the apparent place of the comet could not be determined for that night.

From the 16. till the 19. as alſo after the 25. there happen'd other impediments, which hinder'd making obſervations. Moreover, in the nights between the 19. and 26, the Abbè could not otherways determine the apparent place of the comet than by comparing the phenomenon with *Venus*; in regard he only employed a ſmall quadrant, whoſe tube was ſcarce an *Engliſh* foot in length. From the vertical altitudes, therefore, both of the comet and *Venus* obſerved at the ſame time, were collected the vertical differences of both, as follows.

D.	H.	M. *p. m.*	Vert. Differ. Deg.	M.
20	7	59	5	22
22	7	0	3	56
23	7	20	3	13
24	6	15	2	30
25	7	30	1	47

On

On the 22d the tail of the comet paſſed along the vertical thread of the micrometer in 1 minute, 7 ſec. of time. The micrometer was fitted to the above teleſcope of *Campani*.

D.	H.	M.		Deg.	M.
24	8	3	*Venus* and the comet appear'd under an angle of	7	35
25	7	50	They appear'd under an angle of	8	55

Obſervations on the Comet *ſeen in* January *and* February 1736-7; *and an* Eclipſe *of the* Sun, Feb. 18, 1736-7 *at* Philadelphia *in* Penſylvania; *by Dr.* Kearſly. Phil. Tranſ. N° 446. p. 119.

ON the 27. of *Jan.* 1736-7, about 6 in the evening, Dr. *Kearſly* obſerv'd a dull ſtar about 3 or 4 degrees above *Mercury*, and a little to the ſouthward of a vertical paſſing thro' that planet; but he took little notice of it then, not thinking of a comet: But by comparing *Mercury*'s place with the fixt ſtars, he afterwards thought it might be a comet.

On the 31. about 6 hours 30 min. *p. m.* he took its diſtance from *Venus*, by a reflecting teleſcope of Mr. *Hadley*'s, 14 deg. 40 min. but by a foreſtaff, 14 deg. 50 min. and a right line paſs'd over the comet, *Venus*, and the *Pleiades*.

The night following about 6 hours 20 min. its diſtance from *Venus* was, by Mr. *Hadley*'s inſtrument, 13 deg. 25 min.

The reſt of his obſervations, by ſuch inſtruments as he then had, being none of the beſt, and the comet growing very dull, are as follows.

D.	H.	M. *p. m.*		Deg.	M.
Feb. 7.	6	47	The comet diſtant from *Venus*	7	40
	7	3	—— from *Aldebaran*	59	40
			—— from *Algenib* (by a foreſtaff)	17	45
			A right line drawn from the comet over *Venus* paſs'd over the bright ſtar in the ſide of *Perſeus*.		

D.	H.	M.		Deg.	M.
Feb. 11.	7	14	The comet diſtant from *Venus*	7	12
	7	20	A right line over the comet, *Venus*, and the head of *Caſſiopeia*.		

Feb.

D.	H.	M.			
Feb. 17	7	20	The comet was in a right line, and to the northward of two ſtars; the diſtance of the ſtars the Dr. ſuppos'd might be 40 min. and the comet from the leaſt of them 30 min: Theſe ſtars, he thinks, were the ſouth node of *Piſces*; brighteſt from *Venus* 10 deg. 20 min. from *Aldebaran* 50 deg. 30 min. as he found it ſet down; but it muſt be very falſe.		
			No ſtar within ſight of the comet (by the teleſcope).		
20	30	7	The comet diſtant from *Aldebaran*	34	☉
			—— from *lucida cap. ariet.*	19	30
21	about 30	8	Wanted about a degree of *occulus ceti*; which was the laſt ſight he had of it.		

The eclipſe could not be well obſerv'd at *Philadelphia*, by reaſon of clouds: The Dr. rectified his clock by one of *Heath*'s large ring-dials.

At 7 hours 18 min. there was a ſmall dent in the ſun's edge; whence the beginning was 1 or 2 minutes ſooner.

Juſt before the end, *viz.* at 10 hours 11 or 12 min. he had a ſight of the ſun again; and there was then a dent in the ſun's edge: So that the end muſt be at 10 hours 13 or 14 min. in the morning.

About the middle of the eclipſe there was a large ſpot near the middle of the enlightned part, which was the north ſide of the ſun.

A Comet *ſeen in* Jamaica *in* January, 1736-7; *by Dr.* Roſe Fuller. Phil. Tranſ. N° 446. p. 122.

AT *Spaniſh* town *Jamaica* there was the appearance of a comet, which was firſt perceiv'd about the 26. of *Jan.* 1736-7; but it muſt, by its plainneſs then, have been viſible for ſome time before.

It was firſt of all in the weſt, ſome degrees below and directly under *Venus*: Every night it appear'd nearer to that ſtar, but inclin'd northerly: In about a fortnight it was parallel to it; and in a week after, it was no more to be ſeen.

A Comet *seen at* Madras, Feb. 1736-7 O. S; *by M.* Sartorius. Phil. Transf. N° 446. p. 122.

FOR 7 days preceding the 9. of *Feb.* 1736-7. O. S. about 7 in the evening there appear'd a dim comet, as it was taken to be: It was seen in the west, under *Venus* to the S. W.

Thro' a telescope of 10 or 11 feet it looked like a dim or pale planet; its tail tended upwards.

An Observation of the Comet *seen at* Lisbon; *by Mr.* G. R. Vanbrugh, *with a Remark by Mr.* Machin. Phil. Transf. N°. 446. p. 123.

JAN. 29. 1736-7 on board the *Burford* Man of War, lying at *Lisbon* at 6 hours 49 min. *p. m.* was seen a comet with a long brush tail; at which time its altitude was found to be 5 deg. 15 min. its distance from *Venus* 18 deg. 5 min. and *Venus*'s altitude was observ'd 20 deg. 40 min. it bore due west.

Mr. *Machin* remarks, that according to Mr. *Bradley*'s observations at *Oxford*, the place at the time mentioned ought to have been in Long. ♓ 13° 21′ ½ Lat. 0° 29′ S. so that there seems to have been some mistake in making this observation.

A Description of some Mammoth's Bones *dug up in* Siberia; *together with Observations thereon; by Dr.* Breynius. Phil. Transf. N° 446. p. 124.

IN *Phil. Transf.* N° 403, 404 Sir *Hans Sloane* has given accounts of elephants teeth and bones found under ground. In 1728 Dr. *Breynius* was busied about the same matter; especially to prove, that the extraordinary large teeth and bones found under ground, and dug up in several places of *Siberia*, by the name of *Mammoth*'s or *Mammut*'s teeth and bones were

1. True bones and teeth of some large animals once living, and

2. That those animals were elephants, by the analogy of the teeth and bones with the known ones of elephants.

3. That they were brought and left there by the universal deluge.

In 1730 Dr. *Messerschmidt* return'd to *Dantzick* from his travels thro' *Siberia*, and was pleas'd to communicate to Dr. *Breynius* some curious draughts of a very large scull, *dens exsertus* and *molaris*, with the *os femoris*, belonging to the animal commonly called *Mammoth*, found in *Siberia*; whereby Sir

Hans

PLATE A.

Fig. I.

Fig. II.

A Scale of 3 Feet to Fig. V.

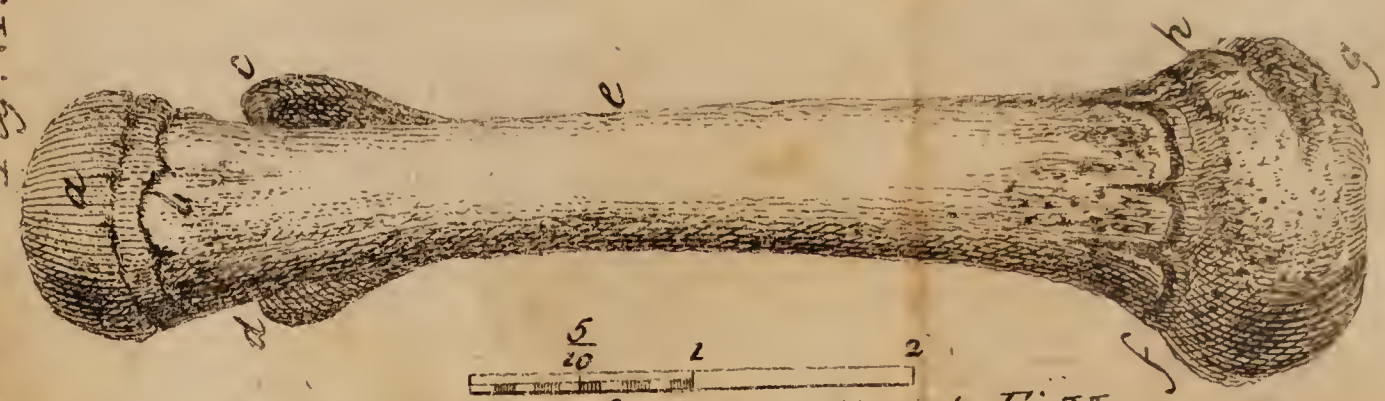
Fig. V.

Fig. IV.

A Scale of 4 Inches or $\frac{4}{10}$ for Fig. IV.

Fig. III.

Fig. VI.



A Scale of 2 Roman Feet to Fig. VI.

A Scale of 2 Roman Feet for Fig. I. II. III.

Fulcher Sculp.

Sir *Hans Sloane*'s and Dr. *Breynius*'s assertion, *viz.* that the teeth and bones, called in *Russia*, *Mammoth*'s bones, are the true teeth and bones of elephants, is not only put in a greater light, but demonstrated beyond all doubt.

In 1722 Dr. *Messerschmidt* found two very large teeth; and upon making an accurate and nice examination of them, one was found to be a *dens molaris*, or grinder, a foot broad, half a foot long, and three inches thick, weighing 8 *lb.* and 3 *oz.* pretty entire, and only broken in two pieces, and the extremities of the roots spoil'd. The substance is between that of a bone and a stone, only that on the upper part of the outside, some parallel undulated lines appear, which still retain the enamel of the tooth.

The other is a piece of a *dens exertus*, or tusk, eight inches long and three inches thick, weighing one pound and six ounces; in some places not differing from ivory, but in others calcin'd like the common *unicornu fossile*.

What *Ysbrand Ides* in his travels from *Mosco* to *China* mentions of the *Mammoth*'s teeth and bones deserves to be taken notice of; as also the journal of *Laurence Lange*'s journey to *China*, to be found in the present state of *Russia*, and the remarks of Capt. *John Bernard Muller*; vide the present state of *Rusland*.

Those above-mention'd, as far as the Dr. knows, are the chief authors that have treated of the *Mammoth*'s teeth and bones, as a very remarkable and particular curiosity of *Siberia*.

From the testimonies of the foresaid authors, it appears,

1. That those teeth and bones are found in *Siberia*, chiefly in the northern parts, near the rivers *Jenizea*, *Trugau*, *Mongam-sea*, *Lena*, &c. towards the icy sea, at the time the ice has broken the banks of those rivers, so that part of the adjacent mountains fall down; and that they are found in such quantity as is sufficient for trade, and to make a monopoly for the *Czar*, vide *the present state of* Rusland.

2. That sometimes skeletons of this kind are found very near complete.

3. That those teeth and bones are not found always of the same size, but sometimes very large, as *dentes molares*, or grinders, of 20 or 24 pound weight, vide Capt. *Muller in loc. citat.* and *dentes exerti*, two of which weigh'd 400 pound; vide *Ysbrand Ides loc. citat.* sometimes of a middle size, as those above-mention'd; and at other times still smaller.

4. That of thoſe teeth, *viz. dentes exerti*, ſome are uſed as ivory, to make combs, boxes, and ſuch other things. Capt. *Muller* ſaith (vide *Tsbrand Ides* and Capt. *Muller in loc. citat.*) that in every reſpect it reſembles the common ivory, being only a little more brittle, and eaſily turning yellow by the weather or heat.

From theſe quoted remarks, joined to ocular inſpection, Dr. *Breynius* thinks he may advance three things.

1. That thoſe *Mammoth*'s teeth and bones are true natural teeth and bones, formerly belonging to very large animals; becauſe they have not only the external figure and proportion, but alſo the internal ſtructure, analogous to the natural teeth and bones of animals.

2. That thoſe large animals have been elephants; which appears by the figure, ſtructure and bigneſs of the teeth, accurately agreeing with the grinders and tuſks of elephants.

To be convinced hereof, one needs only compare theſe teeth with the figures of thoſe, dug up ſome years ago in *Ireland*, and of thoſe which repreſent the very natural teeth of elephants, and conſider the accurate remarks made by Mr. *Molineux* and others of the *Royal Society* thereon.

Nor needs any body doubt, that they are true teeth of elephants, from the uncommon ſize of the *Mammoth*'s teeth above-mention'd; becauſe *Vertomannus*, as the famous Mr. *John Ray* tells us, has ſeen in *Sumatra* a pair of elephants tuſks that weigh'd 336 pound: And *Terzagus*, in *Muſæo Septaliano*, makes mention of one two yards long and 160 pound weight.

3 That thoſe teeth and bones of elephants were brought thither by no other means than that of a deluge, by waves and winds, and left behind after the waters returned into their reſervoirs, and were buried in the earth, even near the tops of high mountains. And becauſe we know nothing of any particular extraordinary deluge in thoſe countries, but of the univerſal deluge of *Noah*, which we find deſcribed by *Moſes*; the Dr. thinks it more than probable, that we ought to refer this ſtrange phenomenon to the ſaid deluge. In ſuch manner, may not only the holy ſcripture ſerve to prove natural hiſtory; but the truth of the ſcripture, which ſaith that *Noah*'s flood was univerſal (a thing doubted by many) may be prov'd again by natural hiſtory.

Here it is to be noted, that ſuch teeth and bones are likewiſe to be found in ſeveral other countries beſides *Siberia*, as *Poland*, *Germany*, *Italy*, *England*, *Ireland*, &c. but leſs common than

in

n *Siberia*, and not ſo well preſerv'd, but more waſted and calcined, without doubt, by the greater warmth of thoſe climates.

Hither are alſo to be referr'd the large bones found under ground, or rather tuſks of elephants, known by the names of ebur, or *unicornu foſſile*, which are of the ſame origin with the *mammoth*'s teeth, but different, as they are better preſerv'd; and therefore for a great part, have ſtill the natural bony ſubſtance, and may ſerve the workmen as natural ivory, and in ſome meaſure the phyſicians and apothecaries as ebur, or *unicornu foſſile*.

The following draughts of the above mentioned antediluvian bones of the animal, commonly call'd the *mammoth* of *Siberia*, or the bones of the foſſile ſkeleton of an elephant, are done to the ancient *Roman* ſcale contracted.

Fig. 1. Plate X. exhibits a front view of the head; it weighs 130 pound, three ounces, five drachms, and one ſcruple apothecary weight, or 152 *Ruſſian* pounds.

Its length or greateſt height is 48 inches.

Its greateſt breadth near the ears, 29 inches, five lines.

Its thickneſs from the forehead to the nape of the neck 22 inches five lines.

a a, the *os frontis*

b b the *ſutura ſagittalis*, hardly diſcernable.

c The bony *ſeptum naſi*, or external proceſs of the *os ethmoides*, without its fellow.

d d The coronal ſuture appearing imperfect.

e e The *oſſa ſincipitis*.

f f The *ſutura ſquamoſa* of the temples.

g g The *ſutura lambdoides* of the occiput.

h The external *proceſſus zygomaticus* of the *os temporum*.

i The poſterior lateral, or zygomatic proceſs of the *os malæs*.

k The upper proceſs of the *os malæ*, join'd with the outer proceſs of the *os frontis*, and conſtituting a part of the orbit of the eye.

l The outer proceſs of the *os frontis*, forming the upper part of the orbit.

m The anterior proceſs of the *os malæ*, join'd with the *os maxillare*.

n n The anterior proceſs of the *os maxillare*, forming the ſockets of the foremoſt teeth

o o The lower lateral proceſs of the *os maxillare*, conſtituting the ſockets of the grinders.

p A grinder in its ſocket, one on each ſide

q A ſurprizing cavity of the noſe, ſtretching above the palate, through which, by means of its *proboſcis*, the water, upon drinking, is convey'd into the throat, in the manner peculiar to the elephant.

Fig. 2. exhibits a view of the right ſide of the head.

a The round proceſs of the *os occipitis*, entering into the *pelvis Atlantis*.

b b The *os occipitis* of a monſtrous ſize.

c c The lambdoidal ſuture.

d The *os petroſum* with the *meatus auditorius*.

e The outer zygomatic proceſs of the *os temporis*.

f The *ſutura ſquamoſa* of the *os temporis*.

g The *os ſincipitis*

h The outer proceſs of the *os frontis*, forming the upper part of the orbit.

i The bottom of the orbit.

k The hole of the optic and pathetic nerves, pointed to by a prick'd line.

l The upper proceſs of the *os malæ*, join'd with the outer proceſs of the *os frontis*, conſtituting part of the orbit.

m The anterior proceſs of the ſame *os malæ*, join'd with the *os maxillare*.

n The poſterior lateral or zygomatic proceſs of the ſame *os malæ*.

o Another zygomatic proceſs of the ſame *os malæ*, peculiar to this skeleton.

p A hole near the foregoing proceſs. *Qu.* If to let a nerve paſs to the teeth?

q q The anterior proceſs of the *os maxillare*, conſtituting the ſockets of the foreteeth.

r r The inferior lateral proceſs of the *os maxillare* ſupporting the ſocket of an upper grinder.

s s A grinder faſt in its ſocket, one on each ſide; which is no ſmall argument that this skeleton belongs to an elephant, and not to the chimerical *Behemoth* of the *Rabbins*, or the *Behæmaeth*, ſuppoſed different from the elephant; of which *Buxtorff*, the learned *Bochart*, and others, have treated.

Fig. 3. Exhibits the back view of the ſame head.

a The great hole of the *os occipitis*, for the paſſage of the *medulla oblongata* to the ſpine.

b b The

b b The *processus globosi* of the *os occipitis*, cover'd with a cartilage, entering into the *pelvis Atlantis*.

c The *os sphenoides cuneiforme*, or *basilare*.

d A peculiar and very remarkable sinus of the *os occipitis*, deeper than an *Ostrich*'s egg, serving in all appearance for the insertion of the muscles of the neck.

e e The outer surface of the *os occipitis* entire.

f f The surface of the same *os occipitis* broken through, exhibiting deep winding cells running on every side.

g The *os petrosum* with the *meatus auditorius*

h Quære if this be the place behind the ears, wherein elephants are wont to be kill'd, and where this one was damag'd by the knife?

i The outer zygomatic process of the *os temporis*.

k The outer process of the *os frontis*, constituting the upper part of the orbit of the eye.

l The bottom of the orbit, and the hole that gives passage to the optic and pathetick nerves, mark'd by a small line.

m The upper process of the *os malæ*, join'd with the process of the *os frontis*, and making up a part of the orbit.

n The posterior lateral or zygomatic process of the *os malæ*

o Another zygomatic process of the same *os malæ*, peculiar to this skeleton.

p. The lower lateral process of the *os maxillare*, supporting the socket of an upper grinder.

q The transverse process of the *maxillary* bone, or the greater *os palati*, which is very short in the skeleton of an elephant, whose tongue is scarce longer than a man's hand; which leave no room to doubt but this must be the skeleton of an elephant.

r r The upper grinders, one on each side, to which their opposites answer in the lower jaw: And as the elephants grinders are commonly four in number, this circumstance is another proof of Sir *Hans Sloane*'s and the Doctor's opinion.

s. The passage from the nostrils into the proboscis and ending in the fauces, with the *os vomer* very visible; though ill drawn through the neglect of the painter.

t t The anterior process of the *os maxillare*, constituting the sockets of the foreteeth, which are represented in fig. 6.

Fig. 4. represents a grinder, which seems to be the left one of the lower jaw, seen on the outside: It weigh'd 8 lb. nine ounces

ounces, and two drachms apothecary weight, or 10 pound *Ruſſian*.

Its greateſt length is 12 inches.

Its perpendicular height five inches.

Its thickneſs or breadth three inches.

It is made up of above 20 tranverſe *lamellæ*, a finger thick, perpendicularly erect, lying cloſe to one another, and its root compos'd of two apophyſes.

a a The plane ſurface of the exerted part of the grinder, ſcarce making half the length of the tooth, contrary to what is obſerv'd in the grinders of the upper jaw.

b b The ends of the tranſverſe *lamellæ*, terminating in the ſurface of the exerted part, and in this grinder as hard as ſtone.

c c The anterior *lamellæ* not extending to the exerted part; and probably, lying hid either in the ſocket of the *os maxillare*, or under the gums.

d The anterior apophyſis or root of the tooth, not quite entire.

e The poſterior apophyſis or root, broken as the foregoing.

f A deep *ſinus* between the two *apophyſes*.

Fig. 5. repreſents the tusk (by ſome improperly call'd the horn) of the right ſide, having a twofold direction by being bent outwards and backwards, which is peculiar to the male elephant; it being ſtraiter in the female: It is the *ebur foſſile* of the ſhops; and weigh'd 137 lb. one ounce, two drachms and two ſcruples apothecary weight, or 160 pound *Ruſſian*.

Its length, or the exterior circumference of its back part was 136 inches, five lines.

The circumference of the root, where it got clear of the ſocket, was the greateſt, being 18 inches, five lines.

The ſubtended arch from one extremity to the other 55 inches.

The ſame ſubtended arch *a c*, but bigger, being 91 inches.

a The root hollow within, the cavity extending beyond the place mark'd *b*.

b The root riſing above its ſocket, where thickeſt.

c The place where the ſubtended arch was greateſt, being 61 inches.

d The point of the tuſk ſomewhat bent outwards and backwards; tho' this curvature could not be expreſs'd by the painter

painter in a visible manner in the lesser subtended arch of 55 inches.

The tusk answering to the foregoing on the left side, was entirely like that on the right, except the contrary direction of its curvature, and its less weight, on account of having lost its point: For, it weigh'd but 128 lb, eight ounces, two drachms apothecary weight, or 150 pound *Russian*.

Fig. 6. represents the right thigh bone, exhibited to view on its inner side, which turns towards the body: It weigh'd 21 lb. six ounces, five drachms, and two scruples apothecary weight, or 25 lb. *Russian*.

Its perpendicular length 38 inches five lines.

The greatest breadth of its upper head, or *apophysis*, 11 inches.

Its circumference at the middle of the bone about 13 inches.

a The head cover'd with a cartilage, placed on its neck, and inserted in the socket of the *os ischium*, and fastened by means of two ligaments.

b The cervix or neck of the bone

c The upper external or greater *trochanter*.

d The lower internal or lesser *trochanter*.

e The place in the middle of the bone, where the circumference measur'd 13 inches.

f The *Sinus* facilitating the free motion of the *patella*.

g The other process, or inward head, covered with a cartilage, together with its fellow.

h Two vertical *sinus's* in the *tibia*, answering to the external *trochanter*.

The bones of this skeleton, with the ribs, *vertebræ*, &c. thereto belonging, were found in the sandy side of a steep hill, on the eastern bank of the river *Indigirska*, which falls into the northern ocean, not far from the mouth of the rivulet *Wolockowoi rugzei*.

The river *Indigirska* to the east of the river *Jena*, where it runs in its own channel, has not been laid down by M. *Witzen* in his map of the north east part of *Asia*: But its course is described by *Ysbrand Ides* in the map of his travels: And some of these bones are found now and then not only in these parts, but likewise in the sand hills on the rivers *Chatanga*, *Thomas*, *Tobol*, *Irtisch*, &c. all at a good distance from the sea, tho' neither elephants nor chimerical behemoth's have ever been seen in those countries; Wherefore the best judges follow

follow the opinion of Dr. *Woodward*, the *Scheuchzers* and others, in taking them for the bones of *antideluvian* animals.

On the bank of the ſame river, which bank is call'd *Sztaoijahr*, *Michael Wolochowicz* ſaw a piece of ſkin putrified, appearing out of the ſide of a ſand hill, pretty large, very thick, and cover'd with long hair, pretty thick ſet, and of a brown colour, ſomewhat like goats hair; which ſkin he could not take for that of a goat, but of the behemoth, inſomuch as he could not appropriate it to any animal he knew.

A large glandular tumour in the pelvis; and the pernicious effects of crude mercury, given internally; by Dr. Cantwell. Phil. Tranſ. N° 446. p. 139.

ONE *P——r M——n* having been very ill for two or three years before had loſt the uſe of his left leg and thigh, was ſubject to frequent head achs and pains in his bones, but eſpecially his legs: For which as he had been given to women, his phyſicians ſalivated him twice, ſent him to ſeveral hot waters, and gave him all the remedies they could think of, but to no purpoſe. For, his illneſs increaſing he had from time to time great difficulty of making water, and going to ſtool. In this condition he came from *Spain* to *Marſeilles*, and from thence was ſent to the waters of *Balleruc*, of which he drank a great quantity: But as they did not paſs, his phyſician there order'd him ſtrong purges, with clyſters of a decoction of tobacco, and the like. He then began to vomit his excrements; upon which the phyſician to a regiment in *Spain*, order'd him half a pound of crude mercury by the mouth, which made him ſuffer the moſt exquiſite pains; and his belly ſwell'd, and became as ſtiff as a drum. Upon this Dr. *Montagne* was ſent for, who ſoon diſcover'd the error of the preceding practice, by feeling a ſolid body near the *rectum*, which obſtructing the paſſage, hinder'd the clyſter-pipe from entering far enough into the gut. After his departure, the patient was again ordered clyſters, which were injected with a crooked pipe, and ſeveral purges; till at the end of eight days he died, having his belly bigger, ſtiffer and harder than ever.

Upon opening the *abdomen*, with a *Biſtouri*, (it was fill'd with a whitiſh liquor of ſome conſiſtence: The *epiploon* was entirely diſſolv'd, and floated in this liquor like ſo much pus. Upon pouring out this water, the Doctor examined the inteſtines: The *colon* was burſt under

der the ſtomach; and in three other places at its lower part; and ſo was the *cæcum:* The *ileum* was all inflamed, and in one part gangrened. The lips of the ruptures were plaiſter'd with excrement, all beſet with a vaſt number of globules of quick-ſilver; and when the inteſtines were diſengaged and taken out, the quickſilver fell from them in large drops. The other *viſcera* were in their natural ſtate, except the liver, which was gangrened.

The Dr. being very ſollicitous about the tumour, look'd into the *pelvis*, where he found an excreſcence of a prodigious ſize, which fill'd all its left ſide: He took the knife and clear'd all round the tumour: whereby he found the urinary bladder cloſe pent up between the anterior part of the tumour and the *oſſa pubis*, which occaſion'd the ſtrangury the patient had been tormented with. The *rectum*, which lay upon the middle of the *os ſacrum*, was alſo vaſtly preſs'd on by the tumour, which ſeemed to take its riſe from the holes that are in the left ſide of that bone. The ſurgeon, (while the Dr. laid down the knife, in order to ſeparate the *oſſa pubis* with a hatchet) cut out the tumour: Then the Dr. examin'd the *os ſacrum*, which was ſo very ſoft that his fingers enter'd it every where on the left ſide. The tumour was of an ovoid figure, cover'd over with ſeveral membranes; it weigh'd 2 pound and $\frac{1}{2}$; its longeſt axis was five inches and ſomewhat more than three quarters, *French* meaſure; its ſhorteſt 4 inches three quarters. At firſt ſight he took it for a *parenchyma*; but upon diſſection, he found it analogous to the liver in ſubſtance, colour and conſiſtence. Its artery, vein and nerve were very large, and diſtributed thorough its whole ſubſtance: Wherefore the Dr. takes it to have really been one of the conglobate glands of the *pelvis*, whoſe veſſels yielding to the blood impell'd thither, with greater force and in a larger quantity than uſual, on account of the violent exerciſes of dancing, jumping, *&c.* which the patient had very much practis'd, caus'd its increaſe to that enormous ſize. Upon opening it, the Dr. obſerv'd three very apparent diviſions in it: And where the *pſoas* lay over it, and one of the *pyramidales* beat upon it, it was oſſified.

The Dr. preſerv'd it in brandy, and found that the ſmall veſſels that were moſt fill'd with blood preſs'd it out into the interſtices of the neighbouring ones.

The weight the patient conſtantly complain'd of at his left hip; the difficulty he had in going to ſtool, and that of thruſting a ſyringe far enough into the *rectum* to give him a clyſter with

with any ſucceſs; the tumour itſelf, which was eaſily felt upon putting the finger into the *anus*; together with the palſy of the left leg and thigh, might, the Dr. thinks, have given other indications to the phyſicians, than thoſe they took. And the Dr. thinks that the frictions and other heating medicines the patient was plied with, contributed to augment his illneſs.

In fine, the crude mercury the patient ſwallow'd, the vaſt quantity of *Balleruc* water he drank before it, with the ſtrong cathartics taken by the mouth and *anus*, ſeem to have cut him ſhort of ſome months, which he might have liv'd, had he us'd no other remedies than a ſlender relaxing diet.

A Narhual *or Unicorn* Fiſh, *taken in the River* Oſt, *in the* Dutchy *of* Bremen; *by Dr.* Steigertahl. Phil. Tranſ. N° 447. p. 147.

TOwards the latter end of *January*, 1736, N. S. there was taken a ſort of whale, call'd the *narhual* or *ſea-unicorn*, in the river *Oſt*, where it falls into the *Elbe*, near the village *Bellum* in the Dutchy of *Bremen*, four miles from the ſea. There was a great quantity of fat taken out of it, to make *thrann* or whale-oil; but it was obſerv'd that this train-oil had an almoſt intolerable ſtench, by reaſon that this *narhual* feeds on carcaſſes: For, *nar* ſignifies a carcaſs or dead body, according to *Valentini* in his *Muſæum Muſæorum* B. III. ch. 30.

There was ſuch care taken of the ſkin, before the diſſection, that it was cur'd with ſalt and alum; and ſtuffed in ſuch manner as to exhibit the juſt figure of the fiſh; having left with it the bones of the ſcull, and ſome *vertebræ* near the tail.

The ſkin was ſpotted with dark brown ſpots upon a white ground: The *epidermis* was tranſparent; and under it was another ſkin very thin and ſpotted; but the true ſkin was brown, and near an inch in thickneſs. On the top of the head was only found a ſemilunar hole, as in the porpoiſe, according to the deſcription given by *John Daniel Major*, and publiſh'd in the *Miſcellan. Academ. Nat. Curioſ. Dec.* 1. *An.* 3. p. 22 *& ſeq.* This hole opens into the two canals running thorough the ſcull to the palate, and are called by *Major*, *ductus hydragogi*. There was no opening or outlet for the *fæces* obſerv'd in the ſkin; and Dr. *Steigertahl* was told, that this *narhual* voided them thorough this hole on the top of the head.

As to the horn, the Dr. agrees in opinion with *Wormius* and others, who take it for a tooth: But he cannot believe that its ſole uſe is to break the ice; it rather ſerving the fiſh for ſeeking its food.

A

A Captain of a *Greenland* veſſel aſſur'd the Dr. that being upon the coaſt a whale-fiſhing, and having taken one; as he was turning the whale to get at the fat, he found on the oppoſite ſide to him, a *narhual*, that had ſtuck his tooth into the whale's belly, up to its mouth, and ſuck'd the blood and humours.

The Dr. only ſaw the ſtuffed ſkin, as it was carrying to *Leipſic* fair: And as he finds that the figure engrav'd and printed at *Hamburg* has a good reſemblance to what he ſaw, he has annex'd a print of it, as repreſented Fig. 1. Plate XI.

1. Shews a ſemilunar hole, thro' which the fiſh upon dying, caſt out water and blood.

2. A ſmall riſing on the middle of the back, and fleſhy as the fins.

3. The mouth very ſmall, without teeth in the upper jaw, except this *dens prominens*, or tuſk, which by ſome has been taken for a horn: There was no lower jaw found.

4. The eye very ſmall.

5. The fin on the right ſide, which, as well as the oppoſite one, is fleſhy.

6. The tail fleſhy like the fins; which taken, according to its width, is not vertical but horizontal.

7. The prominent tooth or tuſk, commonly taken for a horn.

The length of this *narhual* from N° 3 to 6 was 17 feet 9 inches.

The tooth 6 feet 3 inches.

The greateſt thickneſs, meaſur'd round, was 14 feet.

The ſkin was ſmooth without ſcales, like that of an eel, and was white markt with blackiſh ſpots.

A Deſcription of the ſame Narhual; *by Dr.* Henry Hampe. Phil. Tranſ. N° 447. p. 149.

IN a creek, called the *Beluhmer wadt*, belonging to the *Bailiwick* of *Newhaus* in the Dutchy of *Bremen*, was caught alive an unknown fiſh of a large ſize, 18 or 20 feet in length, and 4 in diameter. On the fore-part of the head, juſt above the mouth (which is very ſmall) it has a horn 6 feet long, white like ivory, and curiouſly twiſted. The body is white, ſprinkled with black ſpots, and ſmooth like an eel. The head in proportion to the body, is very ſmall, about 16 inches in length, and the ſame in diameter. The eyes are alſo ſmall, about the bigneſs of a ſixpence. On the upper part of the head is a hole about 3 inches in diameter, out of which he probably

ſpouts out water like the whales. On each ſide of the neck ar placed two black fins, one above the other, and at a ſmall diſ tance from each other. They are half an inch in thickneſ of a hand's breadth and two feet in length, round on the fore part, all fleſhy, and of a liver-colour.

A Water Inſect, *not hitherto deſcribed; by M.* Klein. Phi Tranſ. N° 447. p. 150. *Tranſlated from the* Latin.

A Friend of M. *Klein*'s preſented him with a water inſect found at *Uderwanga* in *Eaſt Pruſſia* among freſh wate crabs, and utterly unknown to the crab fiſhers.

From the great number of its feet, and ſurpriſing facility co moving them, it may with equal, if not better reaſon be call'd *ſcolopendra aquatica ſcutata*, than *Aldrovandus* p. 721. *de cetis* calls a certain fiſh of the whale kind, *ſcolopendra cetacea*.

A (Fig. 2. Plate XI.) repreſents the inſect on its upper part cover'd with a ſhield; which nearly reſembles tortoiſe-ſhells only that along the middle of the back it is a little gibbous, and towards the extremity of the body has a triangular *hiatus* ſlightly indented, it is entire, and almoſt of the ſame ſubſtance (yet of a more dilute colour) with the ſheaths of the wings of the *ſcarabæi Gœdarti*, produced from weevils or what is called *ſcarabæus roſarum*. The eyes paſs out thro' the ſhield and are a little prominent.

B (Fig. 3.) repreſents the inſect delineated on the under part; where at the ſame time appears a vaſt number of legs: Each of theſe has a certain bag as at D (Fig. 5.) terminating in three feet, or rather toes: The two anterior ones have this in peculiar that their three feet or toes are longer than the others, tho' they differ from one another in length. All the toes of the greater and leſſer feet conſiſt of ſimilar articulations; and ſuch as the hairs of the forked tail of this *ſcolopendra*, or the *antennæ* of other inſects have.

M. *Klein* thinks he ſhall not be much miſtaken if he ſuppoſe, this inſect makes uſe of the longer toes of both the anterior feet before, and of the hairs of its forked tail behind for *antennæ*, by means of which it timeouſly diſcovers either its purſuing enemies, or ſuch as it meets with in lurking places; unleſs one would chuſe to take the two ſhort horns, that appear in the Fig. at that place, where we are reaſonably to look for the head, for *antennæ*.

C (Fig. 4.) repreſents the body bared of its ſhield, view'd on the back; on account of which the ſhield is carefully di-

vided

Fig. I.

ig. II.

Fig. III.

Fig. V.

Fig. VI.

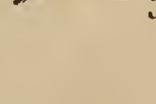
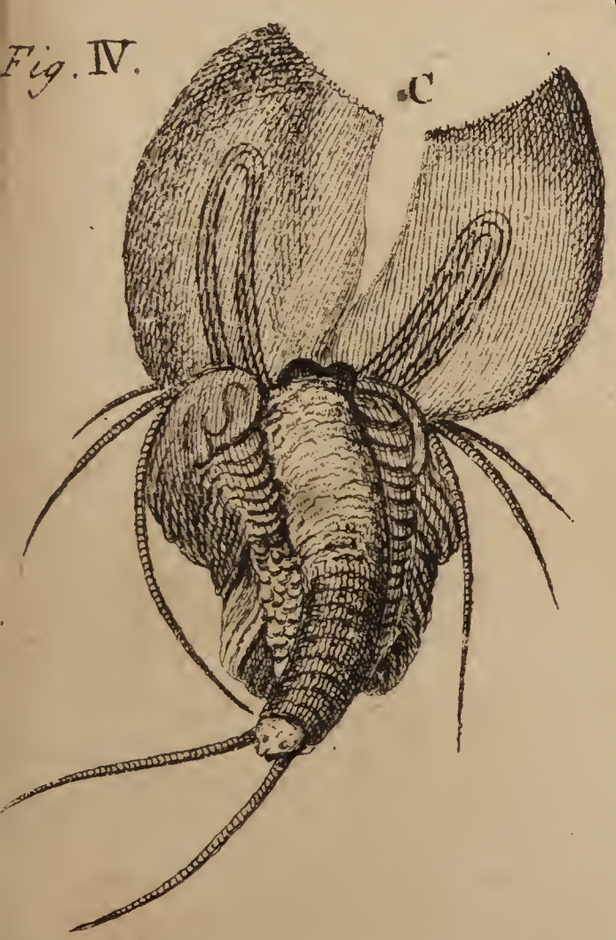


Fig. VII.

Fig. IV.



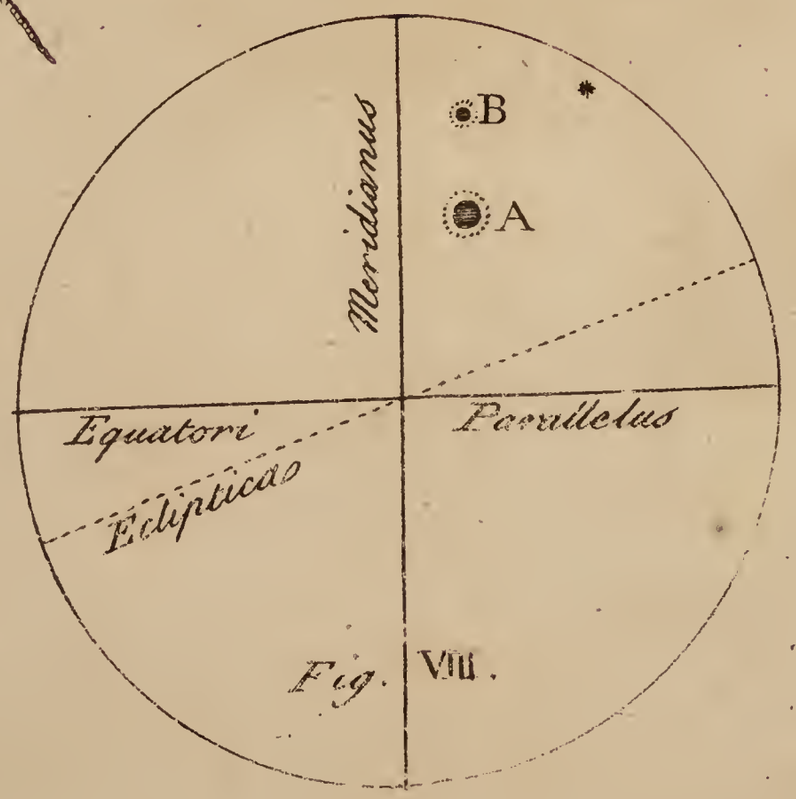


Fig. VIII.

Fig. IX.

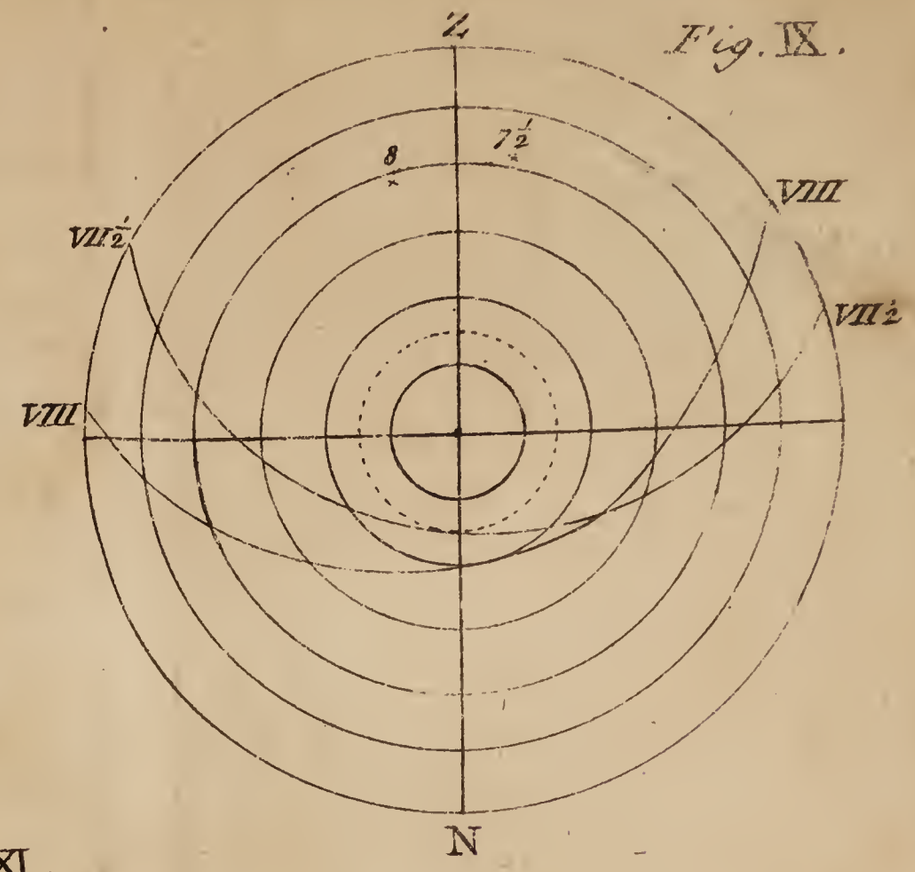
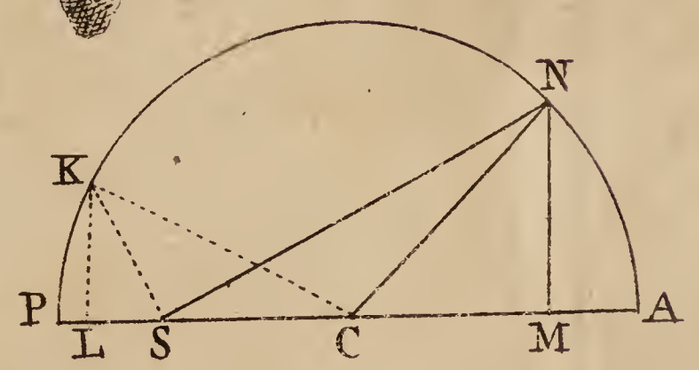


Fig. XI.



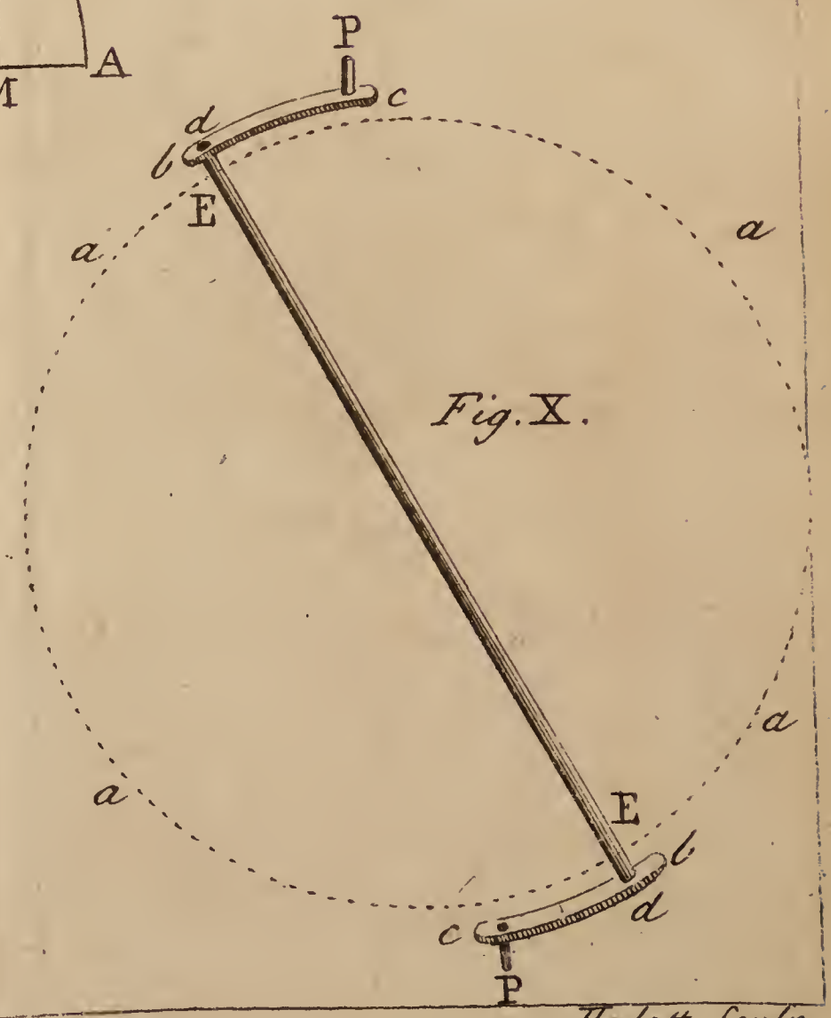


Fig. X.

Hulett Sculp

Smith del.

vided lengthwiſe, which as to the part explain'd is not continu'd with the back, In the thin cuticle of the lower part of the ſhield, and that on both ſides may be obſerv'd (as in the Fig.) punctures like needle work.

M. *Klein* could not certainly determine, whether it ſucks in the water thro' theſe apertures into the cavity between the gibbous ſhield and the cuticle, and again emits it; or whether it fills the cuticle with air, or empties it according as it has a mind either to go down to the bottom or riſe up to the top of the water. The inſections are about 30, but the legs cannot eaſily be numbered. In the extreme part of the body which ſeparates the ſhield, the rings of the inſections are beſet with ſmall ſpines, and that in the ſame order, as they appear delineated Fig. 2 and 4.

D (Fig. 5.) repreſents one of the legs next the anterior ones, together with the little bag.

B (Fig. 3.) repreſents another leg in a different view.

As long as this inſect liv'd, it continually and with ſingular facility mov'd its feet, drawing at the ſame time into its ſheath and putting out again, the extreme part of its body.

M. *Klein* could find no trace of this inſect amongſt authors.

The ſame Sort of Inſect *found in* Kent; *by Mr.* Littleton Brown; *with a Remark*; *by Dr.* Mortimer. Phil. Tranſ. N° 447. p. 153.

MR. *Brown* preſented to the *Royal Society* to be kept in their *Muſæum*, a creature, whoſe name he could not learn from any books or perſons he had hitherto met with: He brought it from a pond upon *Bexby Common* in *Kent*, where great numbers had been obſerv'd for five weeks before. The pond was quite dry the 24th of *June*, 1736; but upon its being fill'd with a great thunder-ſhower on the 25th, within two days the pond was obſerv'd to ſwarm with them: And what Mr. *Brown* thought obſervable was, that there is no duct or channel that could convey them from any adjacent place.

E (Fig. 6. Plate XI.) repreſents this inſect; its legs are very extraordinary. Dr. *Mortimer* counted 42 on a ſide in one of thoſe found in *Kent*; the 20 next the head are nearly of a ſize, but then they grow gradually ſmaller and ſmaller towards the tail. He took out one of the larger ones of the left ſide of the cheſt; the foot conſiſts of five flat membranous claws, with a ſtiff rib along their middle, and beſet with hairs on the edges, like thoſe of crabs. On the lower ſide of the bag hangs an oval

oval bag, and beyond that grows a large thin membrane, which may be extended by a bony rib that runs croſs it; this membrane and the whole foot is convex on the ſide next the head, and concave on that next the tail; the thigh, or firſt joint of the leg, is webb'd on each ſide: So that the whole ſtructure of the legs ſeems to ſhew that they are rather deſign'd for ſwimming than walking.

The leg repreſented at E (Fig. 6.) drawn when the inſect lay on its back as at B. (Fig. 3.) Several parts of this inſect, tho' no bigger than the figures, have ſome reſemblance to thoſe of the *Molucca* crab.

An Abſtract of Meteorological Diaries for the Years 1729, *and* 1730; *by Mr.* Hadley, Phil. Tranſ. N° 447. p. 154.

BEFORE Mr. *Hadley* proceeds to the tables, he gives ſome account of each of the diaries of the years 1729, and 1730, and their contents.

The diary kept by Mr. *Hauksbee*, by order of the *Royal Society*, at their houſe in *Crane Court*, conſiſts of obſervations of the barometrical heights twice a day, *i. e.* morning and evening, in inches, decimals, and centeſimals; as alſo of the thermometer, in its proper graduations, and the weather, with the hour of each obſervation: The winds are omitted. The depth of the rain is ſet down ſeveral times for the moſt part in each month, the ſum of which is to be divided by 10; the funnel which catches the rain being ſo much bigger in ſurface than that of the veſſel which receives the rain from it.

That from *Southwick* near *Oundle* in *Northamptonſhire*, by Mr. *Lynn*, contains the height of the barometer once a day, and the winds, the ſteadineſs and ſtrength of which is likewiſe mark'd in proper characters: Obſervation is made of the upper and under currents of the air, when it ſo happened. The thermometer is mark'd twice a day; the weather often, both by day and night; the rain from time to time; and the quantity of each particular ſhower often ſet down by itſelf, with ſome other miſcellaneous obſervations, as halo's, thunder ſtorms, and ſudden changes of wind, *&c.* He takes notice of his thermometer being placed in an out-houſe expoſed to the air, but ſcreen'd from the ſun, which is a proper precaution in uſing that inſtrument. The remarkable riſes and falls of the mercury are alſo mark'd with proper characters, which method would likewiſe be

uſeful

uſeful in the other columns, for compariſon of diaries, if ſome certain rule were agreed on.

That from *Kent*, 16 miles ſouth eaſt from *London*, gives an account of the barometer once a day; ſometimes twice or thrice, with the hour of each obſervation, and the winds, weather, and rain; the proportion of which for every day is given at the end of each month. There is alſo a ſeparate column for the height of the clouds, which the obſerver divides into three orders; and where there are two orders at a time, they are both noted; as alſo when any of them move with different velocities or directions, which he ſuppoſes to be commonly a ſign of change of the wind: But he does not inform us how he determin'd their heights or velocities. The reigning wind, and general ſtrength of it, is noted at the end of each month; likewiſe the eclipſes, and the times of the new moons, which he obſerves make it appear, that the notion of the change of weather depending on the age of the motion is without any ground; with other miſcellaneous obſervations, as the *aurora borealis*, fruitfulneſs or ſterility of the ſeaſon. He had no thermometer.

That from *Hudickſwall* in *Sweden* by M. *Olave Broman* ſhews the height of the barometer ſometimes once, ſometime twice or thrice a day O S. in *Engliſh* meaſure, with the winds, and their ſtrength, and the weather. There is alſo annex'd to the diary of 1729 an account of the height of the ſea water for every day, which Mr. *Hadley* obſerves to vary in the whole about two inches, and is ſometimes interrupted by rain-floods. This, probably, relates to the tides in the gulph of *Bothnia*. Mr. *Hadley* has not inſerted theſe in the tables, as not being of general uſe. There is no thermometer, nor the quantity of rain ſet down.

That from *Riſinge* in *Oſtrogothia*, in *Sweden*, by *Sueno Laurelius*, paſtor and provoſt gives the height of the barometer for the moſt part three times, ſometimes five times a day, together with the hour of the obſervations O. S. in *Engliſh* meaſure. He refers to the diary of 1727 for the deſcriptions of his barometer and thermometer. The winds together with the degree of their ſtrength, the weather and the depth of rain, are likewiſe ſet down.

In that from *Upſal* in *Sweden*, by M. *Andrew Celſius*, profeſſor of aſtronomy, obſervations are made three times a day of the barometer and thermometer; both which inſtruments were made by Mr. *Hauksbee*; as alſo the winds, with their

their ſtrength, and the weather, and depth of rain from time to time are ſet down.

That from *Svenaker* in *Sweden* near *Trolhetta*, by M. *Torſtanus Waſſenius* contains the height of the barometer twice a day, ſometimes three times O. S. in *Swediſh* feet, inches and decimals, which being ſuppoſed to be to *Engliſh* as 974,375 to 1000, the mean heights are reduced in the following tables to that meaſure. The winds alſo with their ſtrength, and the weather, are noted. There is no thermometer. Notice is taken of thunder ſtorms, and other meteors.

N. B. In the account of the *Swediſh* diaries for 1728, *Svenaker* is ſaid to be 109 *London* feet above the ſurface of the ſea. The mean height of the barometer there in theſe two years, is but 29,47 inches, which would give the height of the place near 450 feet, according to the reckoning hereafter mentioned: Therefore Mr. *Hadley* thinks there muſt be ſome miſtake; perhaps ſome air might have gone into the top of the tube, or the ſcale be placed too high.

That from *Lunden* in *Sweden*, by *Conrad Quenſel* profeſſor of mathematicks in the *Caroline* academy, contains obſervations of the barometer twice a day, O. S in *Engliſh* inches and decimals and fourth parts of them; the winds with their ſtrength, and the weather. The thermometer is *Florentine*; therefore the obſervations are not inſerted in the table. The monthly mean there given, is taken ſimply between the two extremes. Mr. *Hadley* has given it in the tables taken the other way, as all the reſt are.

That from *Bygdea* in *Sweden*, by M. *John Telius*, paſtor there, contains obſervations of the barometer twice a day, morning and evening O. S. in *Engliſh* inches and decimals; the winds, with their ſtrength, and the weather. The two laſt months are wanting. There is no thermometer.

That from *Betna* in *Sudermanland*, by M. *And. Geringius*, paſtor and provoſt, contains obſervations of the barometer thrice a day, except in the firſt part of *January*, O. S. in *Engliſh* inches and decimals, the winds with their ſtrength, and the weather, with other meteorological obſervations; as alſo on the ſeaſons, as to fruitfulneſs and ſterility, *&c.* The *aurora borealis* is frequently mentioned. The thermometer is peculiarly graduated, and ſo could not be inſerted. There is a column for rain, with marks, which Mr. *Hadley* underſtood not.

From

From *Wittemberg* in *Saxony*, there are two diaries communicated; the one from M. *Mat. Haſius*, profeſſor of mathematics; the other from M. *Weidler*: That by M. *Haſius* has the height of the barometer ſeveral times a day, ſometimes four or five times O. S. in *Engliſh* inches and decimals, and the parts of theſe in vulgar fractions, but reduced to decimals in the tables. He employ'd two barometers and thermometers: Thoſe marked I. are Mr. *Haukſbee*'s; thoſe markt II. *Florentine*. The coldeſt day he ever obſerv'd was *Feb.* the 5th 1726. It alſo contains the winds, with their ſtrength, and the weather. M. *Weidler* gives the height of the barometer three times a day, N.S. in *Paris* inches and lines, and the parts of theſe in vulgar fractions; likewiſe the winds, and their ſtrength, and the weather, and the quantity of rain in cubes and lines: but at the end of each quarter the depth is given in *Paris* inches and lines. The thermometer is Mr. *Haukſbee*'s, There are ſome aſtronomical obſervations of eclipſes, &c. He takes notice, that an occultation of *Venus* by the moon, obſerv'd with an 18 foot teleſcope, may ſerve to prove the moon to have an atmoſphere: For, being then in its quadrature with the ſun, it appeared to loſe its cuſps, and become oval, when it came near the moon. Mr. *Hadley* for two years made uſe of M. *Haſius*'s barometrical and thermometrical obſervations, being O. S. and *Engliſh* meaſure, tho' the three laſt months of 1730 are wanting. The depth of rain is taken from M. *Weidler*, and reduced from *Paris* to *Engliſh* meaſure, ſuppoſed to be as 1068 to 1000; but is not reduced to the *old ſtyle*. M. *Weidler* refers to his meteorological obſervations ſent the *Royal Society*. The year 1730 he obſerves to have been more wet and cold than had been known, and the ſky very miſty.

That from *Padua* by S. *Poleni* ſhews the height of the barometer once a day O. S. in *Engliſh* inches and decimals, the winds, and ſometimes their ſtrength, and likewiſe the weather.

A particular account of his thermometer, as alſo the obſervations on his diaries, containing in all ſix years, have been publiſhed in *Phil. Tranſ.* N° 421. The depth of rain is given both for the *old* and *new ſtyle*.

That from *Bengal*, by Mr. *Bellamy*, preacher to the factory, contains the height of the thermometer twice a day, morning and evening, as alſo the winds with their ſtrength, and the weather for the year 1730. The medium of the

thermometer is taken both from the evening and morning heights; the difference there being very considerable in proportion between morning and evening.

That from *Boston* in *New-England*, by Mr. *Dudley*, shews the weather three times a day, and the wind once or twice. There is no barometer or thermometer.

In 1730 those from *Crane-Court*, *Southwick*, *Kent*, *Hudicksvall*, *Ostrogothia*, *Upsal*, *Svenaker*, *Lunden*, *Bettna*, *Wittemberg*, *Padua*, and *Boston*, are continued in the same manner. The *Abo* observations for the year 1730, by Mr. *Sporing*, shew the height of the barometer twice a day in *Swedish* inches and decimals; but the mean heights are reduced to *English* measure in the tables: They likewise shew the winds and weather, and in the last column the *auroræ boreales*, which are frequent in most months of the year.

That from *Naples*, by S. *Cyrillus*, shews the height of the thermometer (which is one of Mr. *Hauksbee*'s) once a day, as also the winds with their strength, the weather and the depth of rain in *Neapolitan* measures, 23 of which make a *London* inch, to which they are reduced in the tables. The barometrical heights he has not set down; because he found them not to agree with those of former years, which made him suspect his instrument to be out of order: But as it appears he had remov'd his habitation, it might be owing to its being situated higher or lower than the former. An eruption of *Vesuvius* happening, an account is given thereof and of damage done by lightning, and likewise of the seasons as to fruitfulness and healthiness, vide *Phil. Trans.* N° 424.

Note, in some of the diaries, the numbers shewing the decimal parts of the inches, are set down in single figures without any rule or cypher to distinguish them from the centesimals, and in others the centesimals in like manner: But it is easy to form a judgment of the authors method by considering them.

Having given an account of the method and contents of the several diaries, Mr. *Hadley* proceeds to the tables extracted from them.

The barometrical table consists of two parts; the upper shews the mean height of the barometer, taken in the method formerly proposed by Dr. *Jurin*, for every month throughout the year, for each place, and in each column the highest month is mark'd with an *h*, the lowest with an *l*, to make them more observable to the eye. At the bottom the mean of the whole year is set down for each place. At the

the foot of this table is another, ſhewing the greateſt aſcent and deſcent of the mercury in that year, with the particular day of each, the difference of which is the range; a circumſtance Dr. *Derham*, and other obſervers, have uſually taken notice of.

Next follows the table of the monthly thermometrical heights, extracted in Dr. *Jurin*'s method alſo in every place where the *Society*'s inſtruments were uſed; and at the bottom the mean of the whole year; and likewiſe the hotteſt and coldeſt day in each place.

In the laſt place are the tables of the depth of rain, where it is contain'd in the diaries.

Mr. *Hadley* choſe to put each of theſe matters in ſeparate tables, that the eye might be able to take a view of the whole, and compare the ſtate of each place with the others, as to every particular, with leſs confuſion; as alſo becauſe ſeveral of the diaries have nothing upon one or more of theſe heads.

Note, The mean heights both of the barometer and thermometer are extracted only from the morning obſervations, ſome of the diaries containing no more; except in the mean heights of the thermometer at *Bengal*, which are taken both from the morning and evening obſervations.

A Table *of the monthly mean* Barometrical *Heights, and also of the greatest Ascents and Descents of the* Mercury *observ'd in several Places, in the Year* 1729, *Inches and Decimals.*

A.

1729	*Crane-Court, London.* Lat. 51° 31	*Southwick* in *Northampterſh.* Lat. 52° 54	In *Kent.* Lat. 51°	*Hudikſval* in *Sweden* Lat. 62°	*Oſtrogothia* in *Sweden* Lat. 56°	*Upſal* in *Sweden.* Lat. 59 48
January	30 08	29 70	29 41 h	29 49	29 20 l	29 43 l
February	02	66	37	30 02	576	92
March	29 93	54	37	29 63	27	55
April	93	60	26	30 04 h	62 h	30 16 h
May	95	57	29	29 82	38	29 71
June	30 07	69	36	86	35	74
July	29 97	64	28	69	20 l	60
Auguſt	30 9 h	72 h	38	78	35	65
September	29 69	42	09	90	42	81
October	83	52	09	55	32	54
November	61 l	32 l	28 94 l	45l	25	54
December	83	52	29 25	75	52	79
Mean of the whole Year.	29 91	29 575	29 ,257	29 ,748	29 371	29 73

C.

*Mer.*High.	30 55 *Feb.* 26	30 ,11 *Feb.* 26	30 36 *Feb* 23	30 70 *Feb.* 28	30 50 *Jan.* 7	30 30 *Dec.* 20
Loweſt	28 31 *Jan.* 18	28 23 *Jan.* 18	28 18 *Oct.* 12	28 75 *Oct.* 12	28 95 *Nov.* 27	28 90 *Nov.* 10
Differ.	2 19	2 17	2 18			

The foregoing Barometrical Table *continued.*

B.

1729	*Svenaker* in *Sweden.* Lat. 50o 10	*Lunden* in *Sweden* Lat. 55. 42	*Bygdea* in *Sweden* Lat. 63. 40	*Betna* in *Sweden.*	*Wittemberg* in *Saxony*, by Mr. *Hasius.* Lat. 52.	*Padua* in *Italy* Lat. 45 15
January	29 44	29 42	29 18	29 77	29 83 h	29 74
February	62	54	76	30 18 h	778	725
March	28	28 38	41	29 84	65	66
April	65 h	57	82 h	30 17	64	85
May	44	48	61	29 89	35	67
June	56	59 h	63	88	77	74
July	43	44	50	721	341	631
August	52	57	57	84	79	79
September	51	44	76	92	65	65
October	37	46	331	76	67	75
November	181	331		75	51	64
December	53	52		99	74	89 h
Mean of the whole Year	29 46	29 47	29 ,557	29 892	29 643	29 727

D.

Mer. High.	30 41 *Feb.* 28	30 ,40 *Feb.* 27.	30 36 *Feb.* 23.	30 70 *Feb.* 18.	30 50 *Jan.* 7.	30 30 *Dec.* 20.
Lowest	28 31 *Jan.* 18	28 23 *Jan.* 18.	28 18 *Oct.* 12.	28 75 *Oct.* 12.	28 95 *Nov.* 27.	28 90 *Nov.* 10.
Diff.	2 10	2 17	2 18	1 95	1 55	1 40

A Table *of the monthly mean* Thermometrical *Heights in several Places in the Year* 1729.

1729.	*Crane-court.*	*Southwick.*	*Upſal.*	*Wittemberg.*
January	67 ,3	70	80	70 ,9
February	69 ,2	74	82 ,7	67 ,5
March	60 8	69	70 ,6	56 ,3
April	55 ,2	61	58, ,9	51 ,6
May	42 ,6	53	48 ,5	36 ,0
June	34 ,9	43	37 ,3	30 ,2
July	33 ,1	42	35 ,1	29 ,0
Auguſt	33 ,8	43	40 ,4	29 ,65
September	38 ,6	46	47 ,1	37, 6
October	51 ,1	57	60 ,8	50 ,2
November	56 ,8	60	66 ,3	59 ,6
December	58 ,8	63	71 ,4	70 ,6
Mean of the Year.	50 ,1	56 ,7	57, 6	49 ,09
Thermom.	12 ,5 *June* 10.	13 *June.* 10.	22 5. *June* 9.	14,75. *May* 23.
Higheſt	80 ,5 *Jan.* 7.	88 *Jan.* 4.	97 *Jan.* 5.	95 ,0. *Jan.* 1.
Loweſt				

A Table of the Depth of Rain which fell in several Places in the Year 1729. in Inches and Decimals.

1729.	Crane-Court.	South-wick.	Kent.	Ostrogo-thia.	Upsal.	Wittemberg Stylo Novo.	Padua.
January	,739	,16	,499	,830	1 ,153		1 ,085
February	,785	,48	1 ,069	1 ,050	1 ,027		1 ,245
March	1 ,125	1 ,31	1 ,286	,600	,826	,48	2 ,902
April	1 ,600	1 ,10	2 ,197	,005	,000	,905	2 ,768
May	1 ,515	1 ,55	2 ,216	3 ,865	,875	,94	2 ,634
June	1 ,200	0, 83	,730	2 ,930	2 ,450	,815	3 ,134
July	1 ,04	2 ,26	2 ,153	1 ,615	2 ,578	1 ,31	4 ,526
August	3 ,04	2 ,44	2 ,533	1 ,405	,747	1 ,365	,578
September	3 ,505	5 ,32	2 ,343	2 ,940	2 ,687	,78	3 ,267
October	1 ,420	2 ,20	2 ,218	1 ,050	,139	1 ,43	6 ,294
November	2 ,425	4 ,18	4 ,334	2 ,150	,855	1 ,305	4 ,186
December	1 ,950	1 ,68	1 ,947	3 ,040	1 ,140	1 ,295	2 ,804
Total.	20 ,344	23 ,51	23 ,525	21 ,480	14 ,477	11 ,625	35 ,423

A Table *of the monthly mean* Barometrical *Heights, and alſo of the greateſt Aſcents and Deſcents of the* Mercury *obſerved in ſeveral Places, in the Year* 1730, *in Inches and Decimals.*

1729.	*Crane-Court, London.* Lat. 51° 31	*Southwick* in *Northamptonſh.* Lat. 52° 54	In *Kent.* Lat. 51°	*Hudickſvall* in *Sweden.* Lat 62°	*Oſtrogothia* in *Sweden.* Lat. 56°	*Upſal* in *Sweden.* Lat. 59° 48
January	30 ,04	29 ,79	29 51 h	29 61	29 50	29 66
February	29 ,61	39	06	50 l	29	60
March	,52 l	34 l	03 l	77	45	79
April	,90	66	37	77	52	77
May	,76	55	15	68	58	72
June	,83	60	24	75	35	75
July	,84	61	31	82	31	78
Auguſt	,94	70	39	75	38	74
September	,90	34 l	37	75	44	75
October	68	49		96 h	64	93 h
November	79	55		50 l	26 l	56 l
December	30 ,09 h	83 h		84	68 h	89
Mean of the whole Year	29 ,825	29 ,57	29 ,27	29 ,725	29 ,455	29 ,745
Mer. High.	30,35 *Dec.* 1. 13.	30,30 *Jan.* 10.	30,01 *Jan.* 10.	30,52 *Dec.* 1.	30,40 *Dec.* 1.	30,71 *Dec.* 1.
Loweſt	28,70 *Mar.* 8.	28,53 *Mar.* 8.	28,28 *Mar.* 8.	28,70 *Dec.* 10.	28;45 *Jan.* 18.	28,90 *Dec.* 10.
Differ.	1,65	1,77	1,73	1,82	1,95	1,81

A. C

The foregoing Barometrical Table *continued.*

1730.	*Svenaker* in *Sweden.* Lat. 58° 10	*Lunden* in *Sweden.* Lat. 55 42	*Betna* in *Sweden.* Lat. 58 49	*Abo* in *Finland.* Lat. 60 40	*Wittemburg* in *Saxony*, by Mr. *Hasius.* Lat. 52	*Padua* in *Italy.* Lat. 45° 15′
January	29 39 l	29 57	29 96	29 68	29 823 h	29 88
February	46	23 l	81	435	428 l	55
March	42	34	97	61	48	46 l
April	59 h	56	30 03	705	677	69
May	54	46	29 89	55	99	67
June	53	55	89	60	667	67
July	40	51	87	665	671	69
August	53	59	89	56	776	80
September	48	57	30 04	30 05 h	80	82
October	56	61	11	29 76		72
November	46	30	29 74 l	355 l		76
December	43	65 h	30 12 h	68		96 h
Mean of the whole Year.	29 ,48	29 ,495	29 ,943	29 ,637	29 ,701	29 ,705
Mer. High.	30,42 *Dec.* 1.	30,22 *Dec.* 1. 13.	30,98 *Dec.* 1.	31,20 *Dec.* 1.	30,37 *Jan.* 30.	30,40 *Dec.* 20.
Lowest	28,43 *Dec.* 10.	28,61 *Dec.* 31.	28,96 *Dec.* 10.	28,64 *Nov.* 23.	29,00 *Feb.* 27.	28,98 *Feb.* 27.
Differ.	1 99	1,61	2,02	1,56	1 37	1 42

A Table of the monthly mean Thermometrical Heights in several Places in the Year 1730.

1730	Crane-Court.	Southwick	Upsal	Wittemberg.	Naples	Bengal
January	67 9	69	74	72	50 5	30
February	63	68	72	67	43	22 ,4
March	56 ,27	62	74 5	60	40	6 ,9
April	50 2	58	56 2	48	40	4 ,0
May	41 30	49	42 4	35	40	5 ,2
June	37 7	46	29 9	29	19 5	5 ,3
July	35 22	42	21 90	26	16 7	8 ,8
August	33 8	44	34 05	29	14 2	9 ,4
September	39 8	47	50 21	40	22 6	6 ,2
October	50 1	55	62 20		29	14 ,4
November	55 5	59	72 27		40 1	23 ,4
December	49 51	70	73 10		49 9	32 ,3
Mean	48 35	55	55 22		34 3	14 ,02
Higheſt	10. 5. July 25	16 July 24.	9 1 July 27.	11 July 25.	9 Aug. 17.	0 Jan.
Loweſt	77. 5. Jan. 7	83 Dec. 14.	86 7 Mar. 16.	81 50 Jan. 15.	60 Jan. 10.	40 Dec..

A Table

A Table *of the Depth of* Rain *which fell in several Places in the Year* 1730. *in Inches and Decimals*

1730	Crane-Court	South-wick	Kent	Ostrogo-thia	Upsal	Wittemberg, Stylo Novo.	Naples	Padua
January	450	45	624	795	164	78	3 89	112
February	1 230	1 53	2 054	870	412	1 68	1 434	2 906
March	3 595	2 61	4 067	2 730	1 983	2 86	739	4 592
April	,670	84	985	605	165	1 98	2 39	1 638
May	1 755	2 5	1 805	2 260	4 120	3 23	1 39	4 467
une	3 755	3 39	2 876	1 531	755	2 31	1 00	6 505
July	2 390	1 93	2 598	2 445	1 904	2 01	2 173	2 339
Auguſt	020	0 85	131	505	525	3 07	0 0	4 269
September	2 100	1 65	2 043	3 140	1 579	2 16	0 67	1 090
October	2 460	2 94	2 424	1 670	1 103	61	2 52	5 254
November	1 570	1 93	2 065	915	831	2 97	2 91	0 534
December	1 500	81	1 322	890	1 105	2 09	3 22	0 894
Total.	21 ,495	21 ,0	22 ,924	18 ,360	14 ,646	25 ,75	24 ,336	34 ,300

Mr. Hadley *makes the following Remarks.*

1. He obſerves on the barometrical tables of theſe two years, *viz.* 1729, and 1730, that they confirm former remarks made by Dr. *Derham*, and others, of the agreement of the barometers in places at a good diſtance from each other: Not only the monthly mean heights agree in the three diaries of the aforeſaid two years in *England*, but alſo the greateſt aſcent and deſcent of the mercury happen commonly on the ſame day; and the barometers have been found to agree in their motions to an hour, ſo far aſunder as *Townley* in *Lancaſhire*, and *Greenwich* near *London*, which is near 160 miles, tho' that might be partly accidental. The barometer at *Crane-Court* and *Southwick*, diſtant about 55 miles, being compar'd, ſeem very ſeldom to vary from their mean difference, above $\frac{1}{10}$ and a half each way; at *Southwick* and *Kent*, ſomething more. From whence it might be expected that the weather ſhould be much the ſame in all theſe places; which nevertheleſs ſeems not to agree with accounts, in ſome years, from different parts in this iſland, not very far diſtant: And Mr. *Hadley* himſelf has ſometimes obſerved clouds to lie in one part of the horizon for a great part of a day, which have diſcharged a large quantity of rain, in places not far off, while the place, where he has been, has all the while enjoy'd fair weather; and *vice verſâ*. Whence it appears, that the barometrical alterations of the air extend farther than their effects, as to the production of rain at thoſe times. Comparing the Diaries of *Crane-Court* and *Upſal*, he finds the barometers vary from the mean difference an inch and a half each way; *Crane-Court* and *Padua* as much, or more, and often go a pace quite contrary at the ſame time; and their monthly differences are alſo very variable; ſo that their agreement at any time ſeems to be but accidental.

2. He obſerves, that the deſcents of the mercury below the mean heights of each place, taken in this way of Dr. *Jurin*, are generally much greater than the aſcents of it above; and there are alſo, in every year, other extraordinary deſcents of the mercury, of the ſame kind. The reaſon he takes to be this; namely becauſe the expanſion of the air, whereby it becomes lighter in ſome one place, being the original of the alterations in the atmoſphere, its effects by condenſation or accumulation of the air in the places round about will be more diſperſed; and therefore leſs ſenſible.

3. The

3. The variation or range is greater the farther north, as has been heretofore obſerved, and appears in theſe tables, in which he has put the Latitude of each place; and likewiſe it is generally greater in the winter than in the ſummer months. The Sum of the motion of the mercury upwards and downwards, taken from the *Berlin* wand'ring line, with a pair of compaſſes, in 1726, amounts to about 76 inches; which gives 5 $\frac{1}{3}$ for a month, and about 0, 21 for each day: but the barometer is by much moſt ſteady in ſummer.

4. The mean height of the barometer hath already been applied to determine the reſpective heights of places, and likewiſe the abſolute height above the ſurface of the ſea. Dr. *Scheuchzer*, in his tables, publiſhed in *Phil. Tranſ.* N° 405, 406, ſuppoſes, from M. *Mariotte*, the mean height at the ſurface of the ſea, to be 28″ 1‴, *Paris* meaſure, which reduced to *Engliſh*, gives 29,993 inches. This agrees very well with a diary, communicated to the *Society*, containing 10 months of the year 1723, and *January* 1724; the author of which found by experiment, that in the place where his barometer was kept, the mercury ſtood $\frac{1}{10}$ and $\frac{1}{2}$ higher than at the ſurface of the ſea, which was not far from the place of his habitation. The mean height of the barometer for thoſe 10 months (leaving out the *January* following, which ſeems to be a very irregular month) Mr. *Hadley* finds to be 29,825, to which adding $\frac{1}{10}$ $\frac{1}{2}$, it will give the mean height at the ſurface of the ſea 29,975: So that the difference between theſe is only 0,018; and therefore, probably, may be near the truth, but may hereafter be more exactly determined by experiments. Then allowing about 90 feet, or rather leſs, for each 10th of an inch in the height of the mercury in ſmaller altitudes, or in greater according to the tables calculated for that purpoſe by Dr. *Scheuchzer* and Dr. *Nettleton*, and publiſh'd in *Phil. Tranſ.* N° 388. you will have the height of each place pretty near, provided the obſervations be carefully made, and continued for a ſufficient time: For, the yearly mean heights in one of the places in theſe tables appear to differ near $\frac{1}{10}$ of an inch in theſe two years: And in moſt of them, the laſt of theſe two years exceeds the firſt, two or three hundredth parts; the barometer alſo ought not to be removed to a lower or higher place.

As to the thermometrical tables, and thoſe of the rain, Mr. *Hadley* only remarks, that the thermometers agree, eſpecially

cially as to the hotteſt days in the year, more than might be expected from places at ſuch a diſtance.

The winds are of ſo uncertain and variable a nature, that they require a more than ordinary care and diligence in making the obſervations, and a great length of time, and a compariſon of a vaſt number of them, before any thing can be deduced more than is commonly known; and therefore Mr. *Hadley* only gives this hint, namely, that if the obſervers would take particular notice, in great ſtorms, of the time when the mercury firſt begins to riſe, whether before or after, or in the very height of the ſtorm, it might be a direction to judge, when an abatement or increaſe of it might be expected (if any regular order ſhould be found therein) which might be ſerviceable on ſome occaſions. But if any attempts ſhould be made to lay down any thing certain concerning the riſe and progreſs of the variable winds, it will appear, by conſidering the cauſe of the trade-winds, that on the ſame account the motion of the air will not be naturally in a great circle, for any conſiderable ſpace, upon the ſurface of the earth any where, unleſs in the equator itſelf, but in ſome other line: And in general, all winds, as they come nearer the equator, will become more and more eaſterly; and as they recede from it, more and more weſterly, unleſs ſome other cauſes intervene.

By the continuance of this method, in proceſs of time, a diſcovery may be made of ſome regular courſe in theſe things, which may be of uſe.

A Solar Eclipſe, obſerved in Fleet-ſtreet, London, Feb. 18, 1737; *by Mr.* George Graham. Phil. Tranſ. N° 447. p. 175.

Apparent Time.

	Ho.	Min.	Sec.	
At	2	25	9	p. m. a ſmall impreſſion appear'd on the ſun's limb. Mr. *Graham* judges the beginning to have been about 5 or 6 ſeconds ſooner.
	3	21	28	The middle of the firſt and larger ſpot cover'd.
		29	30	The middle of the ſmaller ſpot cover'd.
		40	4	The cuſps perpendicular.
	4	3	34	The cuſps horizontal.
		35	32	The middle of the larger ſpot emerged.
		38	21	The ſmaller ſpot emerged, or a little before.

Apparent

Apparent Time.
Ho Min. Sec.

4	52	57	The chord between the cuſps —	1057
	55	0	The chord — — —	954
	56	32	The chord — — —	851
	59	34	The chord — —	632

Then a cloud cover'd the upper limb, and intercepted the view of the ending, which was ſoon after.

Between 12 and 1 o' clock, Mr. *Graham* meaſured the ſun's diameter, with a micrometer.

At the time of the greateſt obſcuration, the lucid part of the ſun's diameter was equal to 392 ſuch parts as his whole diameter contain'd 2188.

Mr. *Graham* had a tranſit of the ſun at noon, and of *Sirius* at night; which, compared with preceding tranſits, he found his clock went about one ſecond in a day too faſt for mean ſolar time.

The ſame Eclipſe obſerved at the Royal Obſervatory, *at* Greenwich, *in company with Dr.* Halley, *by Dr.* Bevis. Phil. Tranſ. N° 447. p. 176.

Apparent Time.
Ho. Min. Sec.

At	2	25	39	p. m. The beginning.
	5	3	29	The end.

At the end, the ſun's limb appear'd ſomewhat tremulous, and a ſmall thin cloud came over it. Dr. *Bevis* judged the time might be relied on to two or three ſeconds.

The ſame Eclipſe obſerved at Edinburgh, &c. *by Mr.* Colin Maclaurin. Phil. Tranſ. N° 447. p. 177.

In the hiſtory of eclipſes, collected by *Riccioli*, there are very few ſaid to be annular; and of theſe ſome have been controverted, as that ſeen by *Clavius*, at *Rome*, *April* 9. 1567, and that ſeen by *Jeſſenius*, at *Torgau* in *Miſnia*, *Feb.* 25, 1598; which are both diſputed by *Kepler*.

Some Aſtronomers, both ancient and modern, have been of opinion, that no eclipſe can be annular: And ſince ſuch ſeem to have been rarely obſerved, and Mr. *Maclaurin* has not met with a particular deſcription of any of them, he gives as full an account of this eclipſe, as he can collect from the obſervations made at *Edinburgh*, and thoſe communicated to him from the country.

During

During the eclipſe, the ſky was generally favourable in the ſouthern parts of *Scotland*; and tho' there were great ſhowers of ſnow in the north, they had ſometimes a view of it. There was ſomething very entertaining in the annular appearance; a phenomenon equally new to all who ſaw it, that gave great delight to the curious, without ſtriking terror into the vulgar. It extended ſouthwards almoſt to *Morpeth* in *Northumberland*, and beyond *Inverneſs* northwards: So that a part of *England*, and almoſt all *Scotland*, were within its limits. Mr. *Maclaurin* had not hitherto learned how far the north limit was from *Edinburgh*; but he was inform'd, that the weather was very unfavourable there.

Ten days before the eclipſe, Mr. *Maclaurin* wrote to ſeveral of his acquaintance in the country, deſiring that they would determine the duration of the annular appearance as exactly as poſſible, in hopes, by comparing their obſervations, to have traced the path of the centre, and the limits of this phenomenon after the example given in 1715 by Dr. *Halley*, to whom we owe the beſt deſcription of an eclipſe that aſtronomical hiſtory affords. Mr. *Maclaurin* gives an abſtract of the accounts he receiv'd in anſwer to theſe letters, after firſt deſcribing the obſervations at *Edinburgh*.

The times of the appearance at *Edinburgh* were determin'd by a *pendulum* clock of Mr. *Graham*'s; from whom likewiſe Mr. *Maclaurin* had the meridian inſtrument by which it is examined. The meridian line was often adjuſted in the uſual manner; and an exact account of the tranſits of the ſun in the meridian, and of *Procyon* in a fixt teleſcope, was kept by Mr. *Short* for a long time before and after the eclipſe: And by comparing his obſervations, Mr. *Maclaurin* cannot doubt but that the times were determined with ſufficient exactneſs; being often with him when he examined the meridian, and obſerv'd thoſe tranſits; particularly the day of the eclipſe, when by the ſun's tranſit in the meridian, it was found that the clock was before the apparent time 13 min. 27 ſec. and ſo much Mr. *Maclaurin* deducted from the times, that were mark'd during the obſervation.

The latitude of *Edinburgh* is commonly ſaid to be 55 degrees 55 minutes; and by ſome trials, this muſt be near the truth, tho' in ſome maps and tables it be repreſented greater. By comparing an obſervation had at *Edinburgh* of the end of the lunar eclipſe, *Nov.* 20. 1732. with an obſervation of the end of the ſame eclipſe by Mr. *Geo. Graham* at his houſe in *Fleet-*

Fleetſtreet, London, the longitude of the former is a little more than 12 minutes of time farther weſt.

Some days before the eclipſe the Lord *Aberdour* ſet up a clock in the caſtle of *Edinburgh*, and adjuſted it with Mr. *Maclaurin*'s by a watch that ſhew'd the ſeconds. The clocks were both compared together the day of the eclipſe at noon, by a cannon fir'd from the caſtle; ſome perſons being appointed to attend each clock, and mark the ſeconds when they heard the ſound (an allowance of two ſeconds and a half being made for the progreſs of the ſound, which was determined by ſeveral trials at night) the clock in the caſtle was found to be before the apparent time 12 min. 19 ſec. and ſo much is ſubducted from the times that were mark'd in the caſtle during the obſervation. It was agreed to give ſignals to one another mutually at the beginning and end of the eclipſe, and at the beginning and end of the annular appearance, which was a cannon from the caſtle, and a muſquet from the college, tho' no regard is had to theſe ſignals in marking the times of the appearances.

His Lordſhip made uſe of a reflecting teleſcope of 15 inches and a half focal diſtance, that magnified 90 times: only he obſerv'd the annular appearance with one of five inches and a half, that he might have a view of the whole diſk of the ſun at once. Mr. *Short* obſerv'd the beginning of the eclipſe with a teleſcope of 15 inches and a half focal diſtance, that magnified 104 times; but the annular appearance with one of the ſame length, that alſo took in the whole disk of the ſun, and magnified 50 times. The reflecting teleſcope, with which Mr. *Maclaurin* obſerv'd the eclipſe from the beginning to the end, took in the whole disk of the ſun (having been made by Mr. *Short* for this purpoſe) tho' the focal diſtance of the big ſpeculum was nine inches and a half; and tho' it bears a higher charge, he made uſe of an eye glaſs on this occaſion, that magnifies only 50 times.

By a computation made at *Edinburgh* from Sir *Iſaac Newton*'s theory, Mr. *Maclaurin* expected the eclipſe would begin at two hours ſix minutes apparent time: they therefore look'd attentively towards the ſouth-weſt part of the ſun's limb from two o'clock. At two hours five minutes 36 ſeconds was perceiv'd a depreſſion that was juſt diſcernable on the ſun's limb near that place; the ſignal at the college was then made, but by an accident my Lord *Aberdour* had been hinder'd from obſerving the ſun at that time: However when he look'd for it he ſaw it was begun, and his ſignal gave general intimation

of this to the town, about 40 ſeconds after they had firſt perceiv'd it at the college; and as far as Mr. *Maclaurin* learned, it was not diſcerned by the eye, tho' aſſiſted with a ſmoak'd glaſs, till about this time.

Mr. *Maclaurin* obſerv'd the progreſs of the eclipſe by a helioſcope; but after 10 digits were eclipſed, he returned to the teleſcope, to attend the beginning of the annular appearance. A little before the annulus was compleat, a remarkable point or ſpeck of pale light appeared near the middle of the part of the moon's circumference, that was not yet come upon the disk of the ſun; and a gleam of light, more faint than this point, ſeem'd to be extended from it to each horn: He did not mark the preciſe time when he firſt peceiv'd this light, but he is ſatisfied that it could hardly be leſs than one fourth of a minute before the annular appearance began. Mr. *Short* (who was in another chamber at ſome diſtance, and made uſe of a larger teleſcope) aſſured Mr. *Maclaurin* that he ſaw it 20 ſeconds before the *annulus* was compleated.

Mr. *Maclaurin* was ſurpriz'd with this light at firſt; and did not immediately recollect, that it proceeded probably from the ſame crown that was ſeen about the moon in a total eclipſe of the ſun at *Naples* in 1605; and was obſerv'd by ſeveral perſons in different parts of *Europe* in the three late total eclipſes of 1706, 1715, and 1724. He did not expect to have ſeen this light, when ſo much of the ſun's disk was uncover'd; but as he kept in the teleſcope only ſo much of the disk as was neceſſary for aſcertaining the time of the formation of the *annulus*, this muſt have contributed to his diſcovering it: For, this light was very faint, compared with what appeared on the ſun's arch near the ſame place the moment it was uncover'd, and the *annulus* compleated.

Moſt of thoſe, who obſerv'd the eclipſe with teleſcopes, mention in their letters to Mr. *Maclaurin*, that as the *annulus* was forming, they perceiv'd the light to break in ſeveral irregular ſpots near the point of contact, and that the moon's limb ſeem'd to be indented there. Some expreſs themſelves, as if thoſe irregular parts had appeared to them in a kind of motion.

In a letter to lord *Aberdour*, it is deſcribed by Mr. *Bayne*, profeſſor of the municipal law in the following manner:

'What appeared to me moſt entertaining, *ſays he*, con-
'ſider'd as an object of ſight was, when the extremities of the
'horns, formed upon the face of the ſun, ſeem'd, as if they had
'been in the action of uniting their points, the inequalities on
'the

'the extremity of the moon's disk exhibited the appearance 'as of small bodies in particular motion.'

There was not any undulation at this time on the circumference of the sun. Mr. *Maclaurin* finds that such appearances of a tremulous motion in certain periods of solar eclipses are mention'd by *Hevelius* and others.

Lord *Aberdour* observ'd the beginning of the annular appearance with a smaller telescope, and perceiv'd only a narrow streak of a dusky red light to tinge the dark edge of the moon, immediately before the ring was compleated, and after it was dissolv'd.

At three hours, 25 minutes, 55 seconds, the circumference of the sun appeared compleat, and perfectly circular. At the same instant the signal from the castle was made, and in a second or two the cannon from the castle was heard. The *annulus* appear'd to the eye to be central for some time; but in the telescope it was always broader towards the south east than towards the north-west part of the sun's disk. The breadth appeared much greater to the naked eye than could have been expected from the difference of the semi-diameters of the sun and moon. This was so remarkable, that such a phenomenon must have confirmed those astronomers in their opinion, who imagined that the diameter of the moon is contracted in her conjunctions with the sun. This appearance proceeded chiefly, Mr. *Maclaurin* supposes, from the light's incroaching on the shadow, as is usual: But whatever was the cause, every body seem'd surpriz'd that the moon appear'd so small upon the sun's disk.

It was observed that the motion of the moon appear'd more quick in the formation and dissolution of the *annulus*, than during its continuance. This is particularly describ'd by Mr. *Fullerton* in a very exact account of the eclipse, as it appear'd at his seat, at *Crosby*, near *Air*, on the west coast of *Scotland*.

'The *annulus* appear'd to be nearly of an uniform breadth 'during the greater part of the time of its continuance, but 'seem'd to go off very suddenly: So that when the disk of the 'moon approach'd to the concave line of the sun's disk, they 'seem'd to run together like two contiguous drops of water on 'a table when they touch one another;' *and he adds*, 'that it 'came on in the same way.'

This appearance seems to be accountable from the same optical deception as the former.

During the appearance of the *annulus*, the direct light of the sun was very considerable; but the places, shaded from his light,

light, appear'd gloomy. There was a dusk in the atmosphere, especially towards the north and east. In those chambers that had not their lights westwards, the obscurity was considerable. *Venus* appear'd plainly, and continued visible long after the *annulus* was dissolv'd; and Mr. *Maclaurin* was told that other stars were seen by some: One Gentleman is positive, that being shaded from the sun, he discerned some stars northwards, which he thinks by their position were in *Ursa major*.

It was very cold at this time; a little thin snow fell, and some little pools of water in the College area, where there was no ice at two o'clock, were froze at four. A reflecting telescope of a large size, and of a much greater aperture than ordinary, that took in the whole sun, and burned cloth very quickly thro' the tinged glass at the beginning of the eclipse, and on that account could not then be us'd with safety, was that by which Mr. *Short* observ'd the annular appearance. Some curious Gentlemen found, that a common burning glass, which kindled tinder at 3 hours 59 minutes, and burned cloth at 4 hours 8 minutes, had no effect during the annular appearance, and for some time before and after it.

Those things Mr. *Maclaurin* mentions mostly on the report of others: For, during the greater part of this appearance he was observing thro' the telescope the progress of the moon upon the sun's disk.

The first internal contact of the disks, at the formation of the *annulus*, was considerably below the west point of the sun's disk; and the second contact, at the dissolution of the *annulus*, seem'd to be about 10 degrees eastwards from the north point or zenith of the disk: But he did not find that the position of those points of contact could be estimated with exactness on several accounts. The breadth of the *annulus* towards the south east part of the sun's disk was at least double its breadth towards the opposite part, about the middle of this appearance. An *apparatus*, by which he was in hopes of being able to determine those things more accurately, was not ready. He propos'd to have made some estimation of the ratio of the continuance of the annular appearance, where it was central to its continuance at *Edinburgh*, from that of the arithmetical mean betwixt the numbers that should express the proportion of the greatest and least breadth of the *annulus* to the geometrical mean betwixt the same numbers; or from the ratio of the radius to the sine of half the arch, intercepted between the two points

points of internal contact: But he did not obtain these ratio's with sufficient exactness.

At 3 hours 31 minutes 43 seconds, the *annulus* was dissolv'd, after having continu'd 5 minutes, 48 seconds: And here again the signals were heard immediately after one another. The middle of the eclipse was, therefore, at 3 hours, 28 minutes, 49 seconds. In this the time of observation did not agree so well with the time by computation, as in the beginning of the eclipse, the difference being here about four minutes. The irregularities of the moon's surface occasion'd the same appearances, in some measure, as at the formation of the *annulus*.

When Mr. *Maclaurin* returned to the helioscope, there was some time lost in directing it towards the sun; and when he got the image in a due position, there was less than eleven digits eclipsed; and he suspects that it never amounted to full eleven digits. He had no micrometer.

After taking some more digits, he went with Sir *John Clerk* to a neighbouring house, to observe the end of the eclipse. By a signal that was made to the person who attended the clock (two seconds being subducted, that were lost in making the signal) the end was at 4 hours 44 minutes 51 seconds. The wind blew hard at this time; so that the telescope could not be kept very steady, and there was some undulation on the circumference of the sun: But Mr. *Maclaurin* cannot think that the error of this observation can exceed three or four seconds, the circumference of the sun appearing to him complete at that instant.

Mr. *Maclaurin* now subjoins the observations made in the Castle and College in one view; whereby it appears that they agree precisely as to the continuance of the annular appearance (a coincidence that could not have been expected) according to the numbers taken by those who attended the clocks.

	In the College			In the Castle		
	H.	M.	S.	H.	M.	S.
The beginning of the eclipse at	2	5	36			
The beginning of the annular appearance	3	25	55	3	25	53
The end of the annular appearance	3	31	43	3	31	41
The end of the eclipse	4	44	51	4	44	48

By

By Lord *Aberdour*'s obſervations, the lowermoſt and largeſt of the two ſpots that appear'd upon the upper part of the ſun's diſk, was touch'd by the moon at 3 hours, 4 minutes, 40 ſeconds; and this ſpot was entirely cover'd at 3 hours 5 minutes, 19 ſeconds. Mr. *Short* obſerv'd another ſpot at the circumference of the moon, at 2 hours, 24 minutes, 51 ſeconds.

Tho' the obſervations of the digits could not, on ſeveral accounts, be made with ſo much exactneſs, as the preceding, Mr. *Maclaurin* ſubjoins ſome of them

		H.	M.	S.
The ſun was eclipſed	2 digits at	2	21	14
	6 dig.	2	50	54
After the annular appearance	9 dig.	3	45	57
	8 dig.	3	52	55
	7 dig.	3	59	53
	6 dig.	4	6	51

At *Hopeton-houſe*, 9 miles weſt, and a little northwards from *Edinburgh*, the Lord *Hope* obſerv'd the annular appearance to begin at 3 hours, 25 minutes, the end of this appearance at 3 hours, 31 minutes; and the end of the eclipſe at 4 hours, 44 minutes and a half. His Lordſhip was obliged to obſerve the eclipſe at a diſtance from the clock, and to determine the times by a pocket-watch, that had been adjuſted by a very good dial that day at 12 o'clock; but he aſſur'd Mr. *Maclaurin*, that the duration of the annular appearance was ſix minutes, as near as could be judged by a watch that did not ſhew the ſeconds. The moon appear'd to touch the larger ſpot above-mentioned at 3 hours, 4 minutes, and cover'd it in about half a minute. The emerſion of the ſame ſpot was at 4 hours, 13 minutes. A leſſer ſpot, higher on the ſun's diſk, was not cover'd till 11 minutes after the greater ſpot, but appear'd rather ſooner than it.

At *Crosby*, on the weſt coaſt of *Scotland*, about 4 miles north from *Air*, Mr. *Fullerton* obſerv'd the eclipſe to begin at 2 o'clock. A diſtinct *annulus* was formed about 20 minutes after three, which continu'd exactly ſeven minutes, meaſur'd by a pendulum vibrating ſeconds. It appear'd rather broader on the lower verge of the ſun; but the difference muſt have been very ſmall: For, it was but bare diſcernible in a ſpecies of the eclipſe ſix inches over, caſt on a piece of paper behind the eye-piece of a teleſcope ſix foot long. He adds, that the day-light

was

was not greatly obſcur'd, appearing only ſo much dimmer than uſual, as that of the ſun is, when ſeen thro' a very gentle miſt in a fine morning in *April* or *May*.

Sir *Thomas Wallace* found that the annular appearance continued at his houſe near *Lockryan* in *Galloway* five minutes.

From the obſervation at *Crosby*, the centre of the annular *penumbra* ſeems to have enter'd *Scotland* not far from *Irwine*. It proceeded afterwards towards the eaſt, with a conſiderable inclination northwards; and probably left *Scotland* not far from *Montroſe* on the eaſt coaſt: For, Mr. *Auchterlony* found that the annular appearance continu'd there ſeven minutes, as near as he could judge by a common watch. The *annulus* likewiſe appeared to him of an uniform breadth, thro' a common teleſcope. This obſervation, tho' not ſo exact as that at *Crosby*, is however confirmed by that at St. *Andrews*, which ſhall be mention'd hereafter.

Theſe two obſervations at *Crosby* and *Montroſe* were made nearer the path of the centre, than any others that were communicated to Mr. *Maclaurin*.

As to the ſouthern limit of this appearance, the eclipſe was not annular at *Newcaſtle*; and there wanted about 40 degrees of the ſun's limb to appear in order to form an *annulus*, according to the obſervation of Mr. *Iſaac Thomſon*.

The whole duration of the eclipſe was 50 ſeconds leſs by Mr. *Thomſon*'s obſervation than by that made at *Edinburgh*; and the larger ſpot was hid 1 hour, 9 minutes, 35 ſeconds by his obſervation: The digits eclips'd at its immerſion were 7,7; at its emerſion 4,1.

The eclipſe was not annular at *Morpeth*; from whence Mr. *John Wilſon* writes, that the body of the moon appear'd almoſt entirely on that of the ſun; and that to the naked eye, the diſk of the ſun ſeem'd to be almoſt round.

But of all the obſervations that were communicated to Mr. *Maclaurin*, that of Mr. *Long* at *Longframlington* (ſeven computed miles on this ſide of *Morpeth*) determines the ſouthern limit with the greateſt exactneſs. The *annulus*, he ſays, was very ſmall there upon the upper part, and the duration 40 or 41 half ſeconds, meaſur'd by a pendulum 9,81 inches long; from which we may conclude, that the limit was very near this place. This curious obſervation, with ſeveral others, was comunicated by Mr. *Mark* at *Dunbar*.

Mr. *Maclaurin* receiv'd no accounts concerning this appearance from any places on the weſt coaſt of *England*.

At

At *Alnwick* in *Northumberland* the eclipſe was annular; but he had not heard that the time of its continuance was meaſur'd.

At *Berwick* the annular appearance continued betwixt 4 and 5 minutes.

The end of the eclipſe at *Dunbar*, by Mr. *Mark*'s obſervation, was at 4 hours, 48 minutes, 16 ſeconds; but there was ſome miſtake committed in reckoning the vibrations of the pendulum in meaſuring the continuance of the *annulus*.

At St. *Andrews*, Mr. *Charles Gregory* and Mr. *David Young*, Profeſſors in the Univerſity, obſerv'd this appearance to continue preciſely ſix minutes, by a pendulum clock. By a figure of the *annulus*, taken from its image, projected thorough a teleſcope upon a paper ſcreen, the breadth towards the ſouth-eaſt part of the ſun's diſk is rather more than double its breadth towards the oppoſite part.

The obſervation at *Montroſe* has been mention'd above.

At *Aberdeen* the *annulus* was obſerv'd by Mr. *John Stewart*, Profeſſor of Mathematics, for three minutes, two ſeconds: It was almoſt central, when the clouds depriv'd him of any farther view of it; he thinks it probable, that it continued there about ſix minutes. Several Gentlemen who live on the coaſt northwards from *Aberdeen* were deſir'd to obſerve the continuance of the *annulus*; but Mr. *Maclaurin* does not find that any of them ſaw this phenomenon from the beginning to the end.

At *Elgin* the eclipſe was obſerv'd annular at 3 hours, 29 minutes (the larger part of the ring being uppermoſt) by Mr. *Irwin*, who had a view of it for about 30 ſeconds; but by reaſon of intervening clouds could not determine the beginning or end of this appearance.

At *Caſtle-Gordon*, Mr. *Gregory* had one view of the eclipſe while it was annular, but could make no farther obſervation by reaſon of clouds.

At *Inverneſs*, the eclipſe was annular for ſome minutes, as Mr. *Maclaurin* was informed by ſeveral Gentlemen; but they did not meaſure the preciſe time of its continuance.

By the accounts Mr. *Maclaurin* had from *Fort Auguſtus* and *Fort William*, it is doubtful, whether the eclipſe was annular in thoſe places or not.

Fort Auguſtus is at the weſt end of *Lochneſs*, and probably was not far from the northern limit of this phenomenon.

Mr.

Mr. *Maclaurin* had hitherto receiv'd no accounts of this appearance from any place farther northwards, or from any place in the weſt, but thoſe already mention'd. Some Gentlemen in *Argyleſhire*, who obſerv'd this eclipſe, were depriv'd of a view of the *annulus* by the clouds.

Mr. *Walker*, an ingenious Gentleman at *Frazerburgh* on the north coaſt, found, that from the time of the *ring*'s beginning to appear upon the lower and weſtern part of the ſun's diſk, till it began to break on the eaſt and upper part, there were 300 vibrations of a pendulum, or five minutes. The *ring* ſeem'd ſomewhat narrower even at the middle of the eclipſe on the lower part.

This is the ſum of what Mr. *Maclaurin* was able to learn concerning the obſervations of this eclipſe, made in *Scotland*, and in the neighbouring parts of *England*.

He made ſome computations relating to the extent of the annular *penumbra*, and the direction and velocity of its motion; but as he had not a ſufficient number of exact obſervations, by which he might examine them, it would be of little uſe to deſcribe them.

Had the weather been more favourable in the north, and had Mr. *Maclaurin*'s requeſt of having the duration of the annular appearance meaſur'd, been made more publick before the eclipſe, after Dr. *Halley*'s example in 1715, he doubts not but he ſhould have been able to have given a more exact account of the progreſs of the centre of this phenomenon, and of its limits; but he was diſcouraged from publiſhing any thing concerning it, by the bad fortune in ſeveral late eclipſes, of which the clouds had not allow'd the leaſt view.

Mr. *Maclaurin* was informed, that there was very little notice taken of this eclipſe by the populace in the country; and he cannot but add, that ſeveral Gentlemen of very good credit, who are not in the leaſt ſhort-ſighted, aſſur'd him, that about the middle of the annular appearance they were not able to diſcern the moon upon the ſun, when they look'd without a ſmoak'd glaſs, or ſomething equivalent.

Mr. *Maclaurin* takes notice of this; becauſe it may contribute to account for what at firſt ſight appears ſurpriſing, namely, that there are ſo few annular eclipſes in the liſts, collected by authors.

Kepler in his *Aſtron. Optic.* does not ſeem to acknowledge, that any eclipſe, truly annular, had ever been obſerv'd.

There are none mention'd by *Riccioli* from the year 334 till 1567; tho' there are 13 or 14 total eclipſes recorded within that period: Yet it is allow'd that the extent and duration of the annular appearance may be conſiderably greater in the former, than of the darkneſs in the latter. It may have contributed to this, that annular eclipſes muſt have been rather incident in the winter ſeaſon in the northern hemiſphere, and that eclipſes have been more readily total in the ſummer, when their chance of being viſible was greater, and the ſeaſon more favourable for obſerving them. But perhaps the chief reaſon, why few annular eclipſes appear upon record, is, that in moſt caſes they have not been diſtinguiſh'd from ordinary partial ones. The darkneſs diſtinguiſh'd total eclipſes, or ſuch as were very nearly ſo; and it is theſe chiefly that hiſtorians mention.

There are two central eclipſes of the ſun ſtill famous amongſt the populace in *Scotland*; that of *March* 29, 1652 was total there, and that day is known amongſt them by the appellation of *Mirk Monday*.

The memory of the eclipſe of *Feb.* 25, 1598. is alſo preſerv'd amongſt them, and that day they term, in their way, *black Saturday*. There is a tradition that ſome perſons in the north loſt their way in the time of this eclipſe, and periſh'd in the ſnow.

There was a remarkable total eclipſe of the ſun in *Scotland*, *June* 17, 1433. the memory of which is now loſt amongſt the populace; but it appears from a paſſage in a manuſcript in the College-library at *Edinburgh*, that after their uſual manner it was formerly call'd by them, the *black hour*. It is deſcribed thus:

'Hoc anno fuit mirabilis eclipſis ſolis, 17^mo die menſis *Junii*, hora quaſi tertia poſt meridiem; & per dimidium horæ tenebræ tanquam in nocte ſupergreſſæ ſunt ſuperficiem terræ; ita ut nihil obtutibus humanis pervium fuit; unde abhinc vulgariter dicta fuit *hora nigra*.'

This eclipſe is not in *Riccioli*'s catalogue, but is mention'd by him in another place, *Schol. cap.* 2. *L.* 5.

By a computation of this eclipſe, the ſun was within two degrees of his *apogæum*, and the moon within thirteen degrees of her *perigæum*: So that this muſt have been a remarkable eclipſe. The progreſs of the ſhadow was towards the ſouth-eaſt; and *Sethus Calviſius* cites the *Turkiſh* annals for its being total in ſome part of their dominions.

On *Feb.* 25. in the evening was look'd for the occultation of *Aldebaran* by the moon; but the ſtar paſſed by the upper horn,

horn, without being hid, at a diſtance from it, that was by eſtimation nearly equal to the diſtance betwixt the neareſt part of the ſpots *Eudoxus* and *Ariſtoteles*.

An Account of the Obſervations of the Solar Eclipſe, made at Edinburgh, *on* Feb. 18. 1736-7. *by Sir* John Clerk. Phil. Tranſ. N° 447. p. 195.

THE 18th of *February*, 1736-7 was a very fine bright day for obſerving the eclipſe; and never was any thing of that kind, Sir *John Clerk* believes, obſerv'd with more exactneſs.

In ſeveral places for 10 miles round the city of *Edinburgh*, as well as in it, were ſome skillful perſons ſtationed for that purpoſe: Sir *John* happen'd to be in the caſtle, which is an eminence of at leaſt 5 or 600 feet in height, beſides a great aſcent from the level of the ſea to the foot of the rock, upon which it is ſituated.

Mr. *Maclaurin*, profeſſor of mathematics, had placed himſelf at a window in the college; others were ſent where the eclipſe, as was ſuppoſed, would be perfectly central, about 12 or 14 miles farther north.

A gun from the caſtle was fir'd at 22 ſeconds after 12, mean time (or 12 minutes, 22 ſeconds before 12, apparent time) upon which, by agreement, the clocks and watches of the obſervers were adjuſted.

A ſecond cannon was diſcharged preciſely when the eclipſe began, which was at five minutes, 36 ſeconds after two.

A third was diſcharg'd when the annular appearance began, which was 25 minutes, 55 ſeconds after three; its continuance was five minutes, 48 ſeconds.

A fourth cannon was fired at the end of the eclipſe, which was at 44 minutes, 50 ſeconds after four: all reckon'd by apparent time.

There were half a ſcore good reflecting teleſcopes to make theſe obſervations, and the calculations agreed perfectly; ſo that they may be depended upon as very exact.

This was not done as a matter of mere curioſity, but to aſſiſt in aſcertaining the motions of the moon on Sir *Iſaac Newton*'s theory, upon which a good deal of the doctrine of the longitude will depend. Sir *Iſaac*'s calculation, as to the beginning of this eclipſe, was pretty right; but not ſo well as to its central appearance.

Two ſpots in the ſun appear'd very diſtinctly, as they enter'd under the moon's body; one was a little above the central

tral or horizontal line of the ſun, of the ſhape as repreſented in Fig. 7. Plate XI. The other was near the edge, on the eaſt quarter. The firſt, compar'd with the ſun's diameter, was larger than the diſk of our earth; it was dark in the middle, and certainly emitted no fire or light.

The edge of the moon appeared a little ragged or ſcabrous, but not mountainous, becauſe of the ſun's light. There was no conſiderable darkneſs, only the ground was cover'd with a kind of a dark greeniſh colour. Two ſtars appear'd, the planet *Venus*, and another farther eaſtward.

This Account Sir *John* aſſures, is what may be depended upon.

Obſervations of the ſame Solar Eclipſe at Trinity-College, Cambridge, *and at* Kettering; *by Mr.* Charles Maſon. Phil. Tranſ. N° 447. p. 197.

		H.	M.	S.	
The beginning by the clock	— at	2	36	40	
The end	— —	5	14	12	exact

Digits	The Eclipſe obſerved.					
	Increaſing.			Decreaſing.		
	H.	M.	S.	H.	M.	S.
$0\frac{1}{2}$	2	39	30	5	11	50
1	2	43	0	5	9	0
$1\frac{1}{2}$	2	46	40			
2	2	50	25			
$2\frac{1}{2}$	2	54	15	4	59	30
3	2	58	5			
$3\frac{1}{2}$	3	1	55			
4	3	5	50			
$4\frac{1}{2}$	3	9	50			
5	3	14	0	4	42	55
$5\frac{1}{2}$	3	18	10	3	39	10
6	3	22	20	4	35	20
$6\frac{1}{2}$	3	26	30			
7	3	30	40	4	27	40
$7\frac{1}{2}$	3	34	50	4	23	55
8	3	39	30	4	20	10
$8\frac{1}{2}$				4	16	30
9	Clouds.			4	13	0
$9\frac{1}{2}$				4	10	10
10				4	7	50

The

		H.	M.	S.
The leſſer ſpot immerged — at		2	58	50
The greater ſpot begun to immerge —		3	33	5
The middle		3	33	20
The end		3	33	37

Times obſerved at *Kettering*, as follows:

	Ho.	Min.
The beginning — —	2	21
2 Digits — — —	2	36
Centre — — —	3	7
	4	22
End — — —	4	59
Great ſpot immerged —	3	18

N. B. The obſervatory clock was one minute 50 ſeconds too ſlow, which, being added all the way, will give true time.

Moſt of the preceeding obſervations are tolerably exact; but the wind, and other difficulties, make ſome of them a little precarious. The time is nearly true, if the error be corrected depending upon the truth of a meridian teleſcope.

A Solar Eclipſe obſerv'd on the 1ſt of March 1737, *N. S. at the Obſervatory of* Bononia. Phil. Tranſ. N° 447. p. 199. *Tranſlated from the* Latin.

THE beginning of this eclipſe preceeded the calculation upwards of ſeven minutes of time: For, at three ho. 33 min. 36 ſec. ſome part of the edge of the ſun, when view'd through a ſmoak'd glaſs that was fitted to an 11 foot teleſcope, appeared to be indented by the moon, when a little before, namely, at 33 ſeconds, the ſun appeared quite round through the ſame teleſcope: But the calculation made the beginning at 3 ho. 41 min.

Afterwards the digits eclipſed were obſerv'd by tranſmitting a ray of the ſun through a ſix foot teleſcope, and receiving it on a white ſcreen, on which was drawn a circle commenſurate to the image of the ſun, and divided into digits and half digits: But the wind ſhaking the machine did not a little mar the obſervation. The more certain phaſes ſeem to be thoſe that follow:

H.	M.	
3	40	About a digit eclipſed
3	48	Two digits
3	57	Three dig.
4	6	Four dig.
4	15½	Five dig.
4	35	Seven digits
4	45	Seven digits and a half; which ſeem'd to be the greateſt obſcuration.
4	55	Again ſeven dig. the eclipſe now decreaſing.

Afterwards when the ſpecies of the ſun, inclining to the weſt, was ſeen too tremulous and fluctuating, as alſo pretty evidently turn'd oval from a round figure, meaſuring the digits, was forbore as a thing not ſufficiently certain

Some maculæ appear'd in the ſun, particularly three, whoſe poſitions taken from the obſervations made that day at noon, are repreſented in Fig. 8. Plate XI.

With the above-mentioned teleſcope of 11 foot, the occultations of two of them were determined, as follows:

Ho.	Min.	Sec.	
4	23	18	The moon's limb touches the coronn a of the *macula* A
	23	49	It begins to cover the *nucleus* of the *macula* A
	24	25	It hides the whole *nucleus*.
	26	14	It juſt touches the *macula* B.
	26	31	It entirely covers it.

The ſame Eclipſe obſerved from the Aventine Hill at Rome, *by the* Abbé de Revillas. Phil. Tranſ. N° 447. p. 200. *Tranſlated from the* Latin.

THE ſun's ſpecies, tranſmitted through a teleſcope of *Campani*'s, ſix foot in length, was received upon a white ſcreen, on which was drawn a circle equal to the ſpecies, and divided into 12 digits: The phaſes obſerv'd in this manner are as follows:

H.

H.	M.	S.	p. m.
3	43	4	The sun's limb is now found to be a little obscur'd by that of the moon.
	51	50	One digit eclipsed
4	0	40	Two digits
	9	30	Three dig.
	18	20	Four dig.
	27	10	Five digits.
	36	0	Six dig. while the limb of the moon touches the centre of the sun, thick clouds intercept the view of both luminaries, and of the following phases of the eclipse.

A partial Eclipse of the Sun, observed at Wittemberg *in* Saxony, Mar. 1. 1737. N. S. *by M.* Weidler. Phil. Trans. N° 447. p. 201. *Translated from the* Latin.

M. *Weidler* observ'd the phases of the emersion, as represented in Fig. 9. Plate XI.

			H.	M.	S.	
VIII	Dig. eclipsed at	—	4	50	31	p. m.
VII $\frac{3}{4}$	—	—		58	16	
VII $\frac{1}{2}$	—	—	5	1	56	
VII $\frac{1}{4}$	—	—		5	26	
VII	—	—		8	16	
VI $\frac{1}{2}$	—	—		10	16	The sun afterwards inclining to the west became overcast.

The beginning of the eclipse could not be seen by reason of clouds.

A Proposal to make the Poles of a celestial Globe move in a circle round the Poles of the Ecliptic; by Dr. Ebenezer Latham. Phil. Trans. N° 447. p. 201.

DR. *Latham* communicated to the *Royal Society* the following proposal, relating to an improvement in the construction of the globes of the heavens.

As we now have them, they are formed for the present age only, and do not serve the purposes of chronology and history, as they might, if the Poles on which they turn were contriv'd to move in a circle round those of the ecliptic, acording to the present obliquity of the latter. By this means we might have a view of the heavens suited to every period, and that would answer the antient descriptions, those of *Eudoxus*, for instance, who is supposed to have borrowed his from the most early observations; and of *Hipparchus*, &c. Nor

could

could any contrivance better enable the meaneſt reader to judge of the merits of the controverſy about the *Argonautic* expedition, as far as it depends on this: For, it will verify to the ſight the place of the colures, *&c.* at any time.

The Equator in the Doctor's method muſt be fix'd to the brazen meridian, *&c.*

N. B. That globes, to anſwer the end here propoſed, tho' differently conſtructed, had long before been made and publiſh'd by Mr. *Senex*, who gave in to the *Royal Society* the following account of his contrivance.

A contrivance to make the Poles of the diurnal Motion in a Celeſtial Globe paſs round the Poles of the Ecliptic; by Mr. John Senex. Phil. Tranſ. N° 447. p. 203.

THE poles of the diurnal motion do not enter into the globe, but are affix'd at one end to two ſhoulders or arms of braſs, at the diſtance of 23 deg. and $\frac{1}{2}$ from the poles of the ecliptic. Theſe ſhoulders at the other end are ſtrongly faſten'd on to an iron axis, which paſſeth thorough the poles of the ecliptic, and is made to move round, but with a very ſtiff motion: So that when it is adjuſted to any point of the ecliptic, which you deſire the equator may interſect, the diurnal motion of the globe on its axis will not be able to diſturb it.

When it is to be adjuſted for any time paſt or to come, bring one of the brazen ſhoulders under the meridian, and holding it faſt to the meridian with one hand, turn the globe about with the other in ſuch manner, that the point of the ecliptic, which you would have the equator to interſect, may paſs under no degrees of the brazen meridian: Then holding a pencil perpendicular to that point, and turning the globe about, it will deſcribe the equator, as it was poſited at that time; and transferring the pencil to 23 degrees and $\frac{1}{2}$, and 66 degrees and $\frac{1}{2}$ on the braſen meridian, the tropics and polar circles will be deſcrib'd for the ſame time.

By this contrivance the celeſtial globe may be ſo adjuſted, as to exhibit not only the riſings and ſettings of the ſtars, in all ages, and in all latitudes, but the other phenomena likewiſe, that depend upon the motion of the diurnal axis round the annual one.

aaaa (Fig. 10. Plate XI.) repreſents a ſection of the celeſtial globe.

E E a ſtrong iron axis, paſſing thro' the poles of the ecliptic.

bc

bc two ſtrong arms of braſs, ſcrew'd on to the ends of the iron axis at *d*.

P P the axis or poles of the diurnal motion (by which the globe is hung in the braſen meridian) rivetted on to the other ends of the braſs arms, and which may be carried round the poles of the ecliptic, by the iron axis; but with ſo ſtiff a motion, as not to diſturb the diurnal rotation on the poles P P.

The Solution of Kepler's Problem; *by Mr.* Machin. Phil. Tranſ. N° 447. p. 205.

SEVERAL attempts have been made at different times; but if Mr. *Machin* miſtake not, never any hitherto with tolerable ſucceſs, towards the ſolution of the problem, propos'd by *Kepler*; namely, *to divide the area of a ſemicircle into given parts by a line from a given point of the diameter, in order to find an univerſal rule for the motion of a body in an elliptic orbit.*

For, among the ſeveral methods offer'd, ſome are only true in ſpeculation, but are really of no ſervice; others are not different from *Kepler*'s own, which he judged improper: And as to the reſt, they are all ſome way or other ſo limited and confined to particular conditions and circumſtances, as ſtill to leave the problem in general untouch'd.

To be more particular, it is evident, that all conſtructions by mechanical curves are ſeeming ſolutions only but in reality unapplicable; that the roots of infinite ſeries's are, on account of their known limitations in all reſpects, ſo far from affording an appearance of being ſufficient rules, that they cannot well be ſuppos'd as offer'd for any thing more than exerciſes in a method of calculation: And then as to the univerſal method, which proceeds by a continued correction of the errors of a falſe poſition, it is, when duly conſider'd, no method of ſolution at all in itſelf; becauſe unleſs there be ſome antecedent rule or hypotheſis to begin the operation (as ſuppoſe that of an uniform motion about the upper *focus*, for the orbit of a planet; or that of a motion in a parabola for the perihelion part of the orbit of a comet; or ſome other ſuch) it would be impoſſible to proceed one ſtep in it.

But as no general rule has ever hitherto been laid down, to aſſiſt this method; ſo as to make it always operate, it is the ſame in effect, as if there were no method at all. And accordingly in experience it is found, that there is no rule now ſubſiſting but what is abſolutely uſeleſs in the elliptic orbits of

comets: For, in such cases there is no other way to proceed, but that which was made use of by *Kepler*; namely to compute a table for some part of the orbit; and therein examine, if the time, to which the place is requir'd, will fall out any where in that part. So that, upon the whole, Mr. *Machin* thinks it evident, that this problem (contrary to the received opinion) has never hitherto been advanced one step towards its true solution; a consideration which will furnish a sufficient plea for meddling with a subject so frequently handled; especially if what is offer'd shall at the same time appear to contribute towards supplying the main defect.

Lemma 1. The tangent of an arch being given, to find the tangent of its multiple.

Let r be the radius of the circle, t the tangent of a given arch A, and n a given number; and let T be the tangent of the multiple arch $n \times$ A to be found.

Then if $\varsigma\varsigma$ be put for $-rr$ and $\tau\tau$ for $-tt$.

The tangent T will be $\dfrac{\overline{r+\tau}\,|^n - \overline{r-\tau}\,|^n}{\overline{r+\tau}\,|^n + \overline{r-\tau}\,|^n}\,\rho$

Which binomials being rais'd according to Sir *Isaac Newton*'s rule, the fictitious quantities τ and ρ will disappear, and the tangent T will become equal to

$$nt - \frac{n}{1}\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}\cdot\frac{t^3}{r^2} + \frac{n}{1}\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}\cdot\frac{n-3}{4}\cdot\frac{n-4}{5}\cdot\frac{t^5}{r^4} - \&c.$$
$$\overline{1 - \frac{n}{1}\cdot\frac{n-1}{2}\cdot\frac{tt}{rr} + \frac{n}{1}\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}\cdot\frac{n-3}{4}\cdot\frac{t^4}{r^4} - \&c.}$$

This theorem (which Mr. *Machin* formerly found for the quadrature of the circle, at a time when it was not known in *England* to have been invented before) has now been common for many years: For which reason he premises it at present, without any proof, only for the sake of some uses that have not hitherto been made of it.

Corollary 1. From this theorem for the tangent, the sine, suppose Y, and cosine Z of the multiple arch $n \times$ A may be readily found,

For,

For, if y be the sine, and z the cosine of the given arch A, then putting vv for $-yy$, and substituting $\frac{ry}{z}$ for t, and $\frac{rv}{z}$ for τ, and $\frac{rT}{\sqrt{rr+TT}}$ for Y.

The sine Y will be $\frac{\overline{z+v}|^n - \overline{z-v}|^n}{2r^n}$ p.

The cosine Z will be $\frac{\overline{z+v}|^n + \overline{z-v}|^n}{2r^{n-1}}$

Each of these may be differently expreſt in a series, either by the sine and cosine conjointly, or by either of them separately.

Thus Y, the sine of the multiple arch $n \times A$, may be in either of these two forms, *viz.*

$$= \frac{z^{n-1}}{r^{n-1}} y \text{ in } n - \frac{n-1}{2}\cdot\frac{n-2}{3}\cdot{}'A\frac{y^2}{z^2} + \frac{n-3}{4}\cdot\frac{n-4}{5}$$

$${}'B\frac{y^4}{z^4} \&c. \text{ or } = ny - \frac{nn-1}{2\cdot 3rr}{}'Ay^3 - \frac{nn-9}{4\cdot 5rr}{}'By^5 - \frac{nn-25}{6\cdot 7rr}$$

$${}'Cy^7 - \&c.$$

Wherein the letters $'A\ 'B\ 'C$, &c. stand, as usual, for the coefficients of the preceding terms.

The first of these theorems terminates, when n is any integer number, the other (which is Sir *Isaac Newton*'s rule, and is deriv'd from the former by substituting $\sqrt{rr-yy}$ for z) terminates when n is any odd number.

The cosine Z may, in like manner, be in either of these two forms, *viz.*

$$= \frac{z^n}{r^{n-1}} \text{ in } \frac{1-n}{1}\cdot\frac{n-1}{2}\cdot\frac{y^2}{z^2} + \frac{n}{1}\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}\cdot\frac{n-3}{4}\cdot\frac{y^4}{z^4}$$

$$- \&c. \text{ or } = r - \frac{nn}{2rr}{}'Ay^3 - \frac{nn-4}{3\cdot 4rr}{}'By^4 - \frac{nn-16}{5\cdot 6rr}{}'Cy^6$$

$$- \&c.$$

The latter of which terminates when the number n is even, and the other as before, when it is any integer.

Corol 2. Hence the sine, cosine, and tangent of any submultiple part of an arch, suppose $\frac{1}{n}A$, may be determined thus;

The tangent of $\frac{1}{n}A$ will be $\frac{\overline{r+\tau}|^{\frac{1}{n}} - \overline{r-\tau}|^{\frac{1}{n}}}{\overline{r+\tau}|^{\frac{1}{n}} + \overline{r-\tau}|^{\frac{1}{n}}}\rho.$

The sine of $\frac{1}{n}A$ will be $\frac{\overline{z+v}|^{\frac{1}{n}} - \overline{z-v}|^{\frac{1}{n}}}{2r^{\frac{1}{n}}}\rho.$

For, these equations will arise from the transposition and reduction of the former for the tangent and sine of the multiple arch, upon the substitution of t, y, z, and A for T, Y, Z, and $n \times A$.

Cor. 3. Hence regular polygons of any given number of sides may be inscrib'd within, or circumscrib'd without, a given arch of a circle: For, if the number n express the double of the number of sides to be inscribed within, or circumscribed about, the given arch A; then one of the inscrib'd sides will be the double of the sine, and one of the circumscrib'd sides the double of the tangent of the submultiple part of the arch, *viz.* $\frac{1}{n}$A.

Lemma 2. To find the length of the arch of a circle within certain limits, by means of the tangent and sine of the arch.

Let t be the tangent, y the sine, and z the cosine of the arch A, whose length is to be determined; and let ρ, τ, v be expounded as before, then if any number n be taken, the arch of the circle will be always less than $\frac{\overline{r+\tau}|^{\frac{1}{n}} - \overline{r-\tau}|^{\frac{1}{n}}}{\overline{r+\tau}|^{\frac{1}{n}} + \overline{r-\tau}|^{\frac{1}{n}}} \times n.$

and bigger than $\frac{\overline{z+v}|^{\frac{1}{n}} - \overline{z-v}|^{\frac{1}{n}}}{2r^{\frac{1}{n}}} \times n\rho.$

For, if, by the preceeding corollaries, a regular rectilinear polygon be inscrib'd within, and another without, the arch A, each having half as many sides, as is expresst by the number n; then will the former of these quantities be the length of the bow of the circumscrib'd polygon (or the sum of all its sides) which is always bigger; and the latter will be the length of the bow of the inscrib'd polygon, which is always less, than the arch of the circle, how great soever the number n be taken.

Cor. 1. Hence the series's for the rectification of the arch of a circle may be deriv'd. For,

For, by converting the binominals into the form of a series, that the fictitious quantities, ρ, τ, v may be destroy'd; it will appear, that no number n can be taken so large as to make the inscribed polygon so big, or the circumscribed so little as the series $\frac{ry}{z} - \frac{ry^3}{3z^3} + \frac{ry^5}{5z^5} - \frac{ry^7}{7z^7} + \&c.$ in one case; or its equal $t - \frac{t^3}{3r^2} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \&c.$ in the other case.

Wherefore, since the quantity denoted by the sum of the terms in either of these series is always bigger than any inscrib'd polygon, and always less than any circumscribed, it must therefore be equal to the arch of the circle.

Cor. 2. If, in the first of the above series's, the root $\sqrt{rr-yy}$ be extracted and substituted for z, there will arise the other series of Sir *Isaac Newton*, for giving the arch from the sine; namely,

$$y + \frac{y^3}{6r^2} + \frac{3y^5}{40r^4} + \frac{5y^7}{112r^6} + \&c. \text{ or otherwise}$$

$$= y + \frac{1}{1.2.3} \times \frac{y^3}{r^2} + \frac{3.3}{1.2.3.4.5} \times \frac{y^5}{r^4} + \frac{3.3.5.5}{1.2.3.4.5.6.7} \times \frac{y^7}{r^6} + \&c.$$

Scholium. In like manner, as the arches of the polygons serve to determine the arch of the circle; so by comparing the area's of the circumscrib'd and inscrib'd polygons, $\frac{1}{2} nr$ T and $\frac{1}{2} n$ Y Z, the area of the sector of a circle may be found; for if T, Y and Z are the tangent, sine, and cosine of the arch A; then by the second *Lemma*, the area of the circumscrib'd polygon will be found to be

$$\frac{1}{2} n r \rho \times \frac{\overline{r+\tau}|^{\frac{1}{n}} - \overline{r-\tau}|^{\frac{1}{n}}}{\overline{r+\tau}|^{\frac{1}{n}} + \overline{r-\tau}|^{\frac{1}{n}}} = \frac{1}{2} n r \text{ T}.$$

And the area of the inscribed will appear to be

$$\frac{1}{2} n \rho \times \frac{\overline{z+v}|^{\frac{2}{n}} - \overline{z-v}|^{\frac{2}{n}}}{4r^{\frac{2}{n}-1}} = \frac{1}{2} n \text{ Y Z}.$$

But

But upon the expanſion of theſe binomials it will appear, that no number n can be taken ſo large, as to make the one ſo big, or the other ſo little, as the area denoted by the ſeries.

$$\tfrac{1}{2} r \text{ in } t - \frac{t^3}{3 r r} + \frac{t^5}{5 r^4} - \frac{t^7}{7 r^6} + \&c.$$

So that this area being larger than any inſcribed, and ſmaller than any circumſcribed polygon, muſt be equal to the area of the ſector. It may be farther obſerv'd, that as the arch or area is found from the ſine, coſine, or tangent of the arch, by means of the limiting polygons; ſo may the ſine, coſine, or tangent be found from the length of the arch by the ſame method.

Thus if A be the arch whoſe tangent T, ſine Y, and coſine Z, are to be determin'd; then will the tangent

$$T \text{ be} = \frac{A - \frac{1}{1.2.3.} \times \frac{A^3}{r^2} + \frac{1}{1.2.3.4.5.} \times \frac{A^5}{r^4} - \&c.}{1. - \frac{1}{1.2.} \times \frac{A^2}{r r} + \frac{1}{1.2.3.4.} \times \frac{A^4}{r^4} - \&c.}$$

$$\text{Sine } Y = A - \frac{1}{1.2.3.} \times \frac{A^3}{r^2} + \frac{1}{1.2.3.4.5.} \times \frac{A^5}{r^4} - \&c.$$

$$\text{Coſine } Z = r - \frac{1}{1.2} \times \frac{A^2}{r} + \frac{1}{1.2.3.4.} \times \frac{r^3}{A^2} \times \frac{A^4}{r^3} \&c.$$

For, it may be made appear from the firſt *Lemma*, and its corollaries, that if in any of theſe theorems, as ſuppoſe in the 1ſt, the quantity A ſtand for the bow of the circumſcribed polygon, then will the quantity T, exhibited by the theorem be always bigger; but if for the bow of the inſcribed polygon, always leſs than the tangent of the arch, how great ſoever the number n be taken; and conſequently if A ſtands for the length of the arch itſelf, the quantity T muſt be equal to the tangent; and the like may be ſhewn for the ſine, and *mutatis mutandis*, for the coſine.

Theſe principles, from whence Mr. *Machin* has here derived the quadrature of the circle, which is wanted in the ſolution of the problem in hand, happen to be upon another account abſolutely requiſite for the reduction of it to a manageable equation. But he has enlarged (more than was neceſſary to the problem itſelf) on the uſes of this ſort of quadrature by the limiting polygons; becauſe it is one of that kind which requires no other knowledge but what depends

on

on the common properties of number and magnitude: And so may serve as an instance to shew that no other is requisite for the establishment of principles for arithmetic and geometry; a truth which tho' certain in itself, may perhaps seem doubtful from the nature and tendency of the present enquiries in mathematicks: For, among the moderns some have thought it necessary for the investigation of the relations of quantities, to have recourse to very hard hypotheses, such as that of number infinite and indeterminate; and that of magnitudes, existing in *statu fieri*, in a potential manner, which are actually of no bigness. And others, whose names are truly to be reverenced on account of their great and singular inventions, have thought it requisite to have recourse even to principles foreign to mathematicks, and have introduced the consideration of efficient causes and physical powers for the production of mathematical quantities, and have spoken of them, and used them, as if they were a species of quantities by themselves.

N. B. In the following proposition, Mr. *Machin* has, for brevity sake, made use of a peculiar notion for composite numbers, (or such quantities as are analogous to them) whose factors are in arithmetical progression.

The quantity express'd by this notation has a double index; that at the head of the root at the right hand, but separated by a hook to distinguish it from the common index, denotes the number of factors; and that above, within the hook on the left hand, denotes the common difference of the factors, proceeding in a decreasing or increasing arithmetical progression.

Thus the quantity $\overset{\alpha}{\underset{m}{\overbrace{\underbrace{n \pm a}}}}$ (m denotes by its index m on the right hand that it is a composite quantity, consisting of so many factors, as there are units in the number m: and the index α above on the left hand denotes the common difference of the factors, decreasing in an arithmetical progression, if it be positive; or increasing, if it be negative: And so signifies, in the common notion at the composite number or quantity $\overline{n \pm a}\ \overline{n \pm a - \alpha}.\ \overline{n \pm a - 2\alpha}$, and so on.

For example $\overset{2}{\underset{5}{\overbrace{\underbrace{n \pm 5}}}}$ (^{6} is $= \overline{n \pm 5}.\ \overline{n \pm 3}.\ \overline{n \pm 1}.\ \overline{n - 1}.\ \overline{n - 3}.\ \overline{n - 5}$. consisting of six factors, whose common difference is 2. After the same manner $\overset{2}{\overbrace{n \pm 4}}$ (^{5} is $= \overline{n \pm 4}.\ \overline{n \pm 2}.\ n.\ \overline{n - 2}.\ \overline{n - 4}$ consisting of five factors. Ac-

According to which method it will easily appear, that if a be any integer, then $\overset{2}{\smile}\overline{n+2a+1}(2a+2$ will be $=\overline{nn-1}\cdot\overline{nn-9}\cdot\overline{nn-25}$, continued to such a number of double factors, as are expresst by $a+1$, or half the index, which in this case is an even number: So $\overset{2}{\smile}\overline{n+2a}(2a+1$ will be equal to $n.\overline{nn-4}.\overline{nn-16}.\overline{nn-36}$, and so on, where there are to be so many double factors, as with one single factor n will make up the index $2a+1$, which is an odd number.

If the common difference a be an unit, it is omitted:

Thus $\smile n(^6$ is $=n.\overline{n-1}.\overline{n-2}.\overline{n-3}.\overline{n-4}.\overline{n-5}$, containing six factors.

So $6(^6$ is $=6.5.4.3.2.1$; and the like for others.

If the common difference a be nothing, then the hook is omitted; and it becomes the same with the geometrical power

So $\overset{0}{\smile}\overline{n+a}|^m$ is $=\overline{n+a}|^m$, according to the common notation

Proposition 1. An arch less than a semicircle being given together with a point in the diameter passing thro' one of its extremities; to find, by means of the sine of a given part of the arch less than one half, the area of the sector subtended by the given arch, and comprehended in the angle, made at the given point.

Let PNA (Fig. 11. Plate XI.) be a semicircle described on the centre C, and diameter AP; and let PN be the given arch less than a semicircle, and S the given point in the diameter AP passing thro' one of the extremities of the arch NP in P. Then taking any number n bigger than 2, let PK be an arch in proportion to the given arch PN, as unity to the number n: and let it be required to find, by means of the sine of the arch PK, the area of the sector NSP, subtended by the given arch NP, and comprehended in the angle NSP, made at the given point S.

From N and K let fall on the diameter AP the perpendiculars NM and KL, and join CN and CK.

Then let t stand for CP the semidiameter of the circle; for CS the distance of the given point S from the centre; for SP the distance of it from the extremity of the arch thro' which the diameter AP passes; and y for KL the sine of the arch KP in the given circle.

Then

These substitutions being presupposed, the problem is to be divided into two cases; one when SP is less, and the other, when it is greater than the semidiameter CP.

Case 1. If SP be less than CP, then take an area H equal to the sum of the rectangles, expressed by the several terms of the following series, continued at pleasure.

$$\frac{Py}{1} + t + \frac{\overset{2}{\smile}{}^{2}}{\underset{\smile}{3}|^{3}} n+1 \times f \times \frac{y^3}{t^2} + 9t - \frac{\overset{2}{\smile}{}^{4}}{\underset{\smile}{5}|^{5}} n+3 \times f \times \frac{y^5}{t^4}$$

$$+ 9 \times 25\, t + \frac{\overset{2}{\smile}{}^{6}}{\underset{\smile}{7}|^{7}} n+3 \times f \times \frac{y^7}{t^6} + \&c.$$

And the area $\frac{1}{2} n \times H$ will determine the area of the sector NSP at pleasure.

For, the sector PSN, being the excess of the sector NCP above the triangle NCS, will be the difference of two rectangles $\frac{1}{2}$ CP × PN — $\frac{1}{2}$ CS × NM; but PN is the multiple of the arch PK; namely $n \times$ PK; and NM is the sine of that multiple arch; wherefore if for CP be put t, for CS, f, according to the supposition; and if for PK be substituted

$$\frac{y}{1} + \frac{1}{\underset{\smile}{3}|^{3}} \times \frac{y^3}{t^2} + \frac{9}{\underset{\smile}{5}|^{5}} \times \frac{y^5}{t^4} + \frac{9 \times 25}{7|^{7}} \times \frac{y^7}{t^6} + \&c.$$ by *Cor.* 2. *Lem.* 2. and for NM.

$$\frac{ny}{1} - \frac{\overset{2}{\smile}}{\underset{\smile}{3}|^{3}} n \cdot n+1|^{2} \times \frac{y^3}{t^2} + \frac{\overset{2}{\smile}}{\underset{\smile}{5}|^{5}} n \cdot n+3|^{4} \times \frac{y^5}{t^4} - \frac{\overset{2}{\smile}}{7|^{7}} n \cdot n+5|^{6} \times \frac{y^7}{t^6} +$$

&c. according to *Cor.* 1. *Lem.* 1. the area of the sector will appear in a series, as is above determin'd.

But since the number n is greater than 2, and the given arch PN is less than a semicircle; and consequently, KL or y, the sine of the submultiple arch PK is less than the semidiameter CP or t; it may thence be easily prov'd, that the series will approximate to the just quantity of the area, at pleasure.

Cor. 1. Hence if the number n be taken equal to $s + \sqrt{25 + \frac{9p}{f}}$, the ſector NSP will be

$$= \tfrac{1}{2} npy + \frac{n^3 t - n.\,n.n-1.\,p}{12tt} y^3 + \text{****} + \frac{n^5}{1120t^5} y^7 + \&c.$$

For, the numerator of the coefficient of the third term in the ſeries, that determines the area H, namely $\frac{\overline{9t - n + 3}^{2} \times ff}{4}$ is equal to $9t - \overline{nn-1.\,nn-9.}\,f$; which according to the above determination of the number n, will become nothing: Wherefore, if for $t - p$ be put f in the ſecond term, and the value of n be ſubſtituted for n in the third and fourth, the ſeries for the area will appear upon reduction to be as is here laid down.

Cor. 2. Hence the area of the ſector NSP may be always defined nearly by the terms of a cubic equation.

For, the number n, as conſtructed in the former *Corollary*, is always greater than the ſquare root of 10; and conſequently $\frac{y}{t}$ is always leſs than the ſine of one third part of the given arch: So that the fourth term $\frac{n^5}{1120t^5} y^7$, with the ſum of all the following terms of the ſeries, can never be more than a ſmall part of the whole ſector.

Cor. 3. If R ſtand for 57,2957795, &c. degrees (or the number of degrees contain'd in an angle ſubtended by an arch of the ſame length with the radius of the circle) and M be the number of degrees in an angle, which is to 4 right angles, as the area NSP to the area of the whole circle; then will M be

$$= \frac{np}{t} \times \frac{Ry}{t} + \frac{n^3 t - n.\,nn-1.}{6t}\, p \times \frac{Ry^3}{t^3} \text{ nearly.}$$

For, $\frac{M}{R} \times \frac{tt}{2}$ will appear by the conſtruction to be equal to the ſector NSP.

Caſe 2. If SP be greater than CP, then take an area H equal to the ſum of the terms in the following ſeries.

Series $\frac{py}{1} + \frac{t - \overset{\smile}{n + 1}\overset{2}{|^2} \times f}{\overset{\smile}{3}|^3} \times \frac{y^3}{t^2} + 9\frac{t + \overset{\smile}{n + 3}\overset{2}{|^4} \times f}{\overset{\smile}{5}|^5} \times \frac{y^5}{t^4} +$

$9 \times 25\frac{t - \overset{\smile}{n + 5}\overset{2}{|^6} \times f}{\overset{\smile}{7}|^7} \times \frac{y^7}{t^6} +$, &c. and the area $\frac{1}{2} n \times$ H will be the sector, as before.

For, the point S being on the contrary side of the centre to what it was before, it will easily appear, that the change of $+f$ into $-f$ must reduce one case to the other, without any other proof.

Cor. Hence if the number n be taken equal to $\frac{\sqrt{t + f}}{f}$; or in this case $\frac{\sqrt{p}}{f}$, then the series for the sector will want the second term, as in the former it wanted the third.

Definition. The angle, called by *Kepler* the *anomalia eccentri*, is a fictitious angle in the elliptic orbit of a planet, being analogous to the area described by a line from the centre of the orbit, and revolving with the planet from the line of *apsides*; in like manner as the mean anomaly is a fictitious angle, analogous to the area, described by a line from the focus.

Otherwise, if C be the centre, S the focus of an elliptic orbit, described on the transverse axis A P, and the area N S P in the circle be taken in proportion to the whole, as the area described in the ellipsis about the focus, to the whole; then is the arch of the circle P N, or the angle N C P, that which *Kepler* calls the *anomalia eccentri*.

This angle may be measured either from the aphelion, or perihelion; in the following proposition it is supposed to be taken from the perihelion.

Prop. 2. The mean anomaly of a comet or planet, revolving in a given elliptic orbit being given, to find the *anomalia eccentri*.

The solution of this problem requires two different rules, the first and principal one serves to make a beginning for a farther approximation, and the other is for the progression in approximating nearer and neaerr *ad libitum*.

1. The rule for the first assumption. Let t, f, p, stand as before, for the semi-transverse axis of the ellipsis, the semi-distance of the foci, and the *perihelion* distance; then taking the number n equal to $\sqrt{5 + \sqrt{59 + \frac{9p}{f}}}$; let T stand for $\frac{2t}{2p}$ $\sqrt{\frac{2p}{t}}$; and P for $nnt - nn - 1.p$. (or $\frac{p}{t}$T) which constant number, being once computed for the given orbit, will serve to find the angle required nearly by the following rule.

Let M be the number of degrees in the angle of mean anomaly to the given time, reckon'd from or to the perihelion; and supposing R, as before, to stand for 57, 2957, &c. degrees; take the number $N = \sqrt[3]{\frac{3T}{nR}}M$, and let A be the angle whose sine is

$$N\sqrt[3]{\tfrac{1}{2} + \sqrt{\tfrac{1}{4} + \frac{P^3}{N^6}}} + N\sqrt[3]{\tfrac{1}{2} - \sqrt{\tfrac{1}{4} + \frac{P^3}{N^6}}}$$

Then the multiple angle $n \times$ A will be nearly equal to the *anomalia eccentri.*

The truth of which will appear from the resolution of the cubic equation in the last corollary to the preceeding proposition.

Cor. 1. If the quadruple of the quantity $\frac{P^3}{N^6}$ be many times greater, or many times less than unity; or which amounts to the same thing, if the mean anomaly M be many times less, or many times greater, than the angle denoted by the given quantity $\frac{2np}{3t}R\sqrt{P}$ (the one or the other of which two cases most frequently happens in orbits of very large eccentricity) then the theorem will be reduced to a simpler form near enough for use.

Case 1. If M be many times less than $\frac{2np}{3t}R\sqrt{P}$ then the angle A may be taken for that whose sine is $\frac{t \times M.}{np \times R}$

Case 2. If M be many times greater than $\frac{2np}{3t}R\sqrt{P}$; then

then let A be the angle whose sine is $N - \frac{P}{N}$, and the multiple angle $n \times A$, according to its case, will be nearly equal to the angle required.

Cor. 2. In orbits of very large eccentricity, the perihelian distance p is many times less than the semi-distance of the foci f, and the number $n = \sqrt{5 + \sqrt{25 + 9\frac{p}{f}}}$, is always nearly equal to $\sqrt{10}$, or the integer 3, either of which may be used for it without any material error in the orbits of comets.

II. The Rule for a farther correction *ad libitum*.

Let M be the given mean anomaly, t the semi-transverse axis as before; and let B be equal to, or nearly equal to the multiple angle n A before found, then if μ be the mean anomaly, and x the planet's distance from the sun, computed to the *anomalia eccentri* B; the angle $\grave{B}$ taken equal to $B + \frac{t}{x} \times \overline{M - \mu}$ will approach nearer to the true value of the angle sought; and by repeating the same operation, the approximation may be carried on nearer and nearer *ad libitum*.

This last rule being obvious, the explication of it may be omitted.

Scholium. In this solution, where the motion is reckon'd from the perihelion, the rule is universal, and under no limitation: But had the motion been taken from the aphelion, the problem must have been divided into two cases; one is, when the eccentricity is less than $\frac{2}{16}$; the other is when it is not less, but is either equal to, or more than in that ratio.

If the eccentricity be not less than $\frac{2}{16}$, then the same rule will hold as before, only putting the aphelian distance, suppose a, instead of the perihelian distance p, and substituting $-f$ for $+f$ in the rule for the number n.

If the eccentricity be less than $\frac{2}{16}$, then take the number n equal to $\sqrt{\frac{a}{f}}$ and $\frac{t}{n\,a} \times \frac{M}{R}$ will be nearly equal to the sine of the sub-multiple part of the *anomalia eccentri*, denominated by the number n, as before.

It is needless to observe, that the like rules would obtain in hyperbolic orbits, *mutatis mutandis*. But that which perhaps

sort

may not appear unworthy of being remark'd, concerning this ſort of ſolution from the cubic root, is, that tho' the rule be altogether impoſſible upon a total change of the figure of the orbit either into a circle, or into a parabola; yet it will operate ſo much better, aad ſtand in need of leſs correction, according as the figure advances nearer in its change towards either of theſe two forms.

That the uſe of the method may the better appear, Mr. *Machin* adds a few examples; two for the orbits of planets, one the moſt, and the other the leaſt eccentric; but which are more to ſhew the extent of the rule, than to recommend the uſe of it in ſuch caſes: For, there are many other much better and more expeditious methods in orbits of ſmall eccentricity: The other two examples are adapted to the orbits of two comets, whoſe periods have been already diſcover'd by Dr. *Halley*; the one is to ſhew the uſe of one of the rules in the firſt corollary, and the other is to explain the uſe of the other rule.

Ex. 1. For the orbit of *Mercury*.

If an unit be put for the ſemi-tranſverſe axis, t, the eccentricity 0, 20589 will become f, and the perihelian diſtance p will be 0, 79411; wherefore by means of the number R, given as before, the conſtant numbers for this orbit will appear to be

$n = 3, 56755$, $T = 0, 5857271$, $P = \frac{p}{t} T = 0, 4651319$; and hence $\frac{3T}{n \times R} = 0, 0085965$.

Suppoſe M the mean anomaly from the perihelion to be 120°, to which it is requir'd to find the *anomalia eccentri*.

Here ſince the mean anomaly M is not many times more than the limiting angle $\frac{2np}{3t} R \sqrt{P}$ (which in this orbit is about 74 degrees) recourſe muſt be had to the general rule in the propoſition.

The number N then, which is $\sqrt[3]{\frac{3T}{nR}} M$ will be $= 1, 0104195$, which found gives

$N \sqrt[3]{\frac{1}{2} + \sqrt{\frac{1}{4} + \frac{P^3}{N^6}}} = 1, 0389090$, and alſo

$N \sqrt[3]{\frac{1}{2} - \sqrt{\frac{1}{4} + \frac{P^3}{N^6}}} = 0, 4477126$: Wherefore the ſum of both (under their proper ſigns) *viz.* 0, 5911964 will be the ſine, whoſe

whose arch 36°, 24195 is the angle A, the multiple whereof $n \times A = 129°, 295503$ will be the angle to be first assumed for the *anomalia eccentri*.

For a farther correction; this angle, now call'd B, whose sine is (suppose) y, and its cosine z, gives, by a known rule, $t + \frac{f}{t} z = 1, 1304$ for x the planet's distance from the sun; and by another known rule $B - \frac{fR}{tt} y = 120°, 16568$ for μ the mean anomaly to the *anomalia eccentri* B: Wherefore, the correct angle $B = B \times \frac{t}{x} \times \overline{M - \mu}$ will be $129°, 14846 = 129°. 8'. 54'', 5$, erring, as will appear from a farther correction, about a tenth of a second.

This angle, being thus determined, will, by the common methods, give 137° 48′ 33″ and ½ for the true anomaly or angle at the sun; the sine of the true anomaly being in proportion to the sine of the *anomalia eccentri*, as the semi-conjugate axis to the planet's distance from the sun. So that the equation of the centre in this example is 17° 48′ 33″ and ½.

Ex. 2. for the orbit of *Venus*.

Supposing, as before, the mean distance t to be unity, and the eccentricity f to be 0, 0069855; the constant numbers for this orbit will be $p = 0, 9930115$, $n = 6, 4116$, $T = 1, 562134$, $P = 0, 1551217$, $\frac{3T}{nR} = 0, 0127571$, and the limiting angle $\frac{2np}{3t} R \sqrt{P}$ will appear to be about 393 degrees.

Let M be 120°, as in the former example. Then, since the mean anomaly is, in this case, not many times less than the limiting angle, the general rule must be used as before; according to which the number N will appear to be 1, 152585, the sine of A will be 0. 3217917, the angle A 18, 077132, and the multiple $n \times A$ or angle B, for the first assumption of the *anomalia eccentri* will be 120°, 35416.

This angle B will give, by the method before explained, the angle B = 120°, 34555, or 120° 21′ 44″ *fere*, for the *anomalia eccentri* correct; the error of which will, upon examination, appear to be but a small part of a second.

In this example the true anomaly is 120° 41′ 25″, 1; and consequently, the equation of the centre no more than 41′ 25″, 1.

Ex.

Ex. 3. For the orbit of the comet of 1682.

To know the mean anomaly of this comet to any given time, it is to be premiſed, that it was at the perihelion *An.* 1682, on the 4th of *September* at 21 ho. 22 min. equated time to the meridian of *Greenwich*, and performs its revolution round the ſun, as Dr. *Halley* has diſcover'd, in 75 years and a half.

The perihelian diſtance p is, according to his determination, 0, 0326085 parts of the mean diſtance t: So that the conſtant numbers for the orbit will be $n = 3, 1676061$, $T = 0, 2054272$ $P = 0, 00669867$, and the limiting angle $\frac{2np}{3t} R\sqrt{P}$ will be about 19 minutes or $\frac{1}{3}$ of a degree.

In the orbits of comets, the rule for the firſt aſſumption of the *anomalia eccentri* is generally ſufficient without correction.

Thus, ſuppoſe the mean anomaly M to be 0, 072706 (as it was at the time of an obſervation made at *Greenwich* on the 30th of *Auguſt*, 1682, at 7^h 42′ equat. time) then the general rule (which muſt be here us'd, ſince the angle of mean anomaly is not above four or five times leſs than the limiting angle) will give $n \times A$ or $B = 2°\ 12'\ 48'', 7$, erring about $\frac{2}{12}$ of a ſecond from the true *anomalia eccentri*.

But in theſe orbits the rules in the firſt corollary to the ſecond propoſition moſt frequently take place, eſpecially the laſt; and the calculation may alſo be farther abreviated, by putting the ſquare root of 10, or the integer 3, for the number n.

Suppoſe the mean anomaly to be $0°$, 006522, or 23′, 4792; here, ſince M is 50 times leſs than the limiting angle, the rule in the firſt caſe of the firſt corollary may be us'd; that is, to take the ſine of the angle $A = \frac{t \times M}{np \times R}$.

Wherefore, if the number 3 be put for n, the ſine of A, which is $\frac{tM}{3pR}$ will be $= 0, 00116367$; and conſequently, the angle A will be 4′ 0″, 011; and the multiple angle $n \times A$ to be aſſumed for the *anomalia eccentri* will be 12′. 0″ ‘033; the error of which will be found to be about $\frac{1}{10}$ of a ſecond.

Ex. 4. For the orbit of the great comet *An.* 1680.

This comet, according to Dr. *Halley*, performs its period in 575 years; and was in its perihelion on the 7th of *December* 1680, at 23^h 9′ equat. time at *London*, the perihelian diſtance p is 0, 00008930$\frac{1}{2}$, in parts of the mean diſtance t: Wherefore, ſuppoſing the number n to be $\sqrt{10}$, the conſtant numbers for the orbit will be $T = 0, 2000161$, $P = 0, 00001786\frac{2}{3}$, and the limiting

limiting angle $\frac{2npRrP}{3t}$ will be about $\frac{1}{6}$ of a second.

Suppose the mean anomaly to be 3′ 31″, 4478, or 0°,0587354 (as it was at the time of the first observation of it in *Saxony*, *Nov.* 3. at 16^h 47′ equat. time at *London*) here since the mean anomaly is many times greater than $\frac{1}{6}$ of a second, the rule in the second case of the first corollary may be used; that is, by taking the sine of $A = N - \frac{P}{N}$.

But the number N or $\sqrt[3]{\frac{3T}{nR}}M$ is = 0, 05794134, and $\frac{P}{N}$ will be equal 0, 0030827; wherefore $(N - \frac{P}{N} =$ 0, 05763307 will be the sine, whose arch 3°, 30397 is the angle A; and the multiple angle $n \times A = 10°\ 26'\ 53'', 05$ will be the angle to be first assumed for the *anomalia eccentri*; the error of which will be found to be less than a second.

The true anomaly, computed from this angle, according to the rule in the example for *Mercury*, will appear to be 171° 38′ 24″ from the perihelion.

By these examples it appears, that the solution is universal in all respects: For, the two first, compared with the two last, serve to shew that it is not confined to any particular parts of the orbit, but extends to all degrees of mean anomaly: And by comparing the second with the last, it sufficiently appears to be universal with respect to the several degrees of eccentricity; since in one, the equation of the centre for the reduction of the mean to the true motion is not so much as the $\frac{1}{170}$ part of the whole; whereas in the other it amounts to almost 3000 times as much as the mean motion itself.

Upon reviewing the reflections on the quadrature of the circle, Mr. *Machin* thinks it necessary, in order to prevent any mistake, that may arise from the different opinions, that obtain about the nature of mathematical quantity, to explain himself a little upon that head; as also to add a few words to shew, how the method of quadrature by limiting polygons takes place in other figures as well as the circle.

Mr. *Machin* takes then a mathematical quantity, and that for which any symbol is put, to be nothing other than number, with regard to some measure, which is considered as one: For, we cannot know precisely and determinately, that is, mathematically

cally, how much any thing is, but by means of number. The notion of continued quantity, without regard to any measure, is indistinct and confused; and tho' some species of such quantity, considered physically, may be described by motion, as lines by points, and surfaces by lines, and so on; yet the magnitudes or mathematical quantities are not made by that motion, but by numbering according to a measure.

Accordingly, all the several notations, that are found necessary to express the formations of quantities, do refer to some office or property of number or measure; but none can be interpreted to signify continued quantity as such.

Thus some notations are found requisite to express number in its ordinal capacity, or the *numerus numerans*, as when one follows or preceeds another, in the first, second, or third, place from that, upon which it depends; as the quantities $\dot{x}, \ddot{x}, \dot{\dot{x}}, \dot{x}, \ddot{x}$, referring to the principal one x.

So, in many cases, a notation is found necessary to be given to a measure as a measure; as for instance, Sir *Isaac Newton*'s symbol for a fluxion $\dot{x}$: For, this stands for a measure of some kind, and accordingly he usually puts an unit for it, if it be the principal one upon which the rest depend.

So some notations are expresly to shew a number in the form of its composition, as the index to the geometrical power x^n, denoting the number of equal factors, which go to the composition of it, or what is analogous to such.

But that there is no symbol or notation but what refers to discrete quantity, is manifest from the operations, which are all arithmetical.

And hence it is, there are so many species of mathematical quantity, as there are forms of composite numbers, or ways in the composition of them; among which there are two more eminent for their simplicity and universality than the rest: One is the geometrical power, formed from a constant root; and the other, tho' well known, yet wanting a name, as well as a notation, may be call'd the arithmetical power, or the power of a root uniformly increasing or diminishing, and is that whose notation is design'd above, after *Scholium Lemma* 2: The one is only for the form of the quantity itself, the other is for the constitution of it from its elements.

Now from the properties of either of these it would be easy to shew, how the quadratures of simple figures are deducible from the area's of their limiting polygons. Mr. *Machin* just points out the method from the arithmetical power, as being the shortest and readiest at hand.

Let

Let $z, \dot{z}, \ddot{z}$, &c. or $z, \dot{z}, \ddot{z}$, &c. be quantities in arithmetical progression, diminishing or increasing by the common difference $\dot{z}$; and let (as has been before explain'd) $\underset{z}{\dot{z}}(m$ signify the arithmetical power of z, denominated by the potential index m, namely $z \times \overset{'}{z} \times \overset{''}{z}$, &c. whose first root is z and last $z - \overline{m-1} \times \dot{z}$, which being supposed, the element of the arithmetical power will be $m \underset{z}{\dot{z}}(m-1 \times \dot{z}$, that is, the product made from the multiplication of the two indices, and the next inferior power of the next root in order: For, the first arithmetical power $\underset{z}{\dot{z}}(m$ is $= z \cdot \underset{z}{\ddot{z}}(m-1$, and the next $\underset{\dot{z}}{\dot{z}}(m$ is $= \underset{\dot{z}}{\dot{z}}(m-1 \times z - \overline{m-1}\dot{z}$; wherefore the difference will be as is explain'd.

And consequently, since the sum of these elements or differences, taken in order from the first to the last, do make up the quantity, according to its *termini*; hence, if z be the abscis of a curvilinear figure, whose ordinate y is equal to $m z^{m-1}$, a demonstration might easily be made, that the form of the quantity for the area will be z^m; that is, the same multiple of the next superior power of z, divided by the index of that power.

For, since the arithmetical powers do both unite and become the same with the geometrical power, when the differential index $\dot{z}$ is supposed to be nothing; the magnitude of the geometrical figure will be implied from the magnitudes of the two polygons, made up of rectangles, one from the increasing, the other from the diminishing arithmetical power; tho' it be true, that the elements of the polygons cannot be summ'd up, when $\dot{z}$, the measure of the abscis z, is supposed to be nothing.

In like manner, in any other case where z and $\acute{z}$ are two abscisses whose difference as a measure is $\dot{z}$, and $y, \acute{y}$ the two ordinates; the magnitude of the figure will be implied by the magnitudes of the two polygons, which are made from the sum of the inscribing and circumscribing elements $\dot{z}y$ and $\dot{z}\acute{y}$;

tho' the figure itself is not to be resolved into any such primogenial rectangular elements.

And thus, Mr. *Machin* thinks, that the symbol z, consider'd as a component part of the rectangle $z\,y$, may bear a plain interpretation; *viz.* that it is the measure, according to which the quantity z is measur'd; nor can he see that any other interpretation need be put upon a symbol, which, like a measure, is used only to make other things known, but is of itself nothing but a mark.

And what is said of the elements of the first resolution, is easily applicable to those of a second or third, and so on: The last may always be consider'd as the measure of the former, and is indivisible; tho' in respect of the following, it be taken as the part, according to which the measure was made, and therefore be divisible.

A Description of a new invented Water-bellows; *by M.* Triewald. Phil. Trans. N° 448. p. 231.

THE water-bellows M. *Triewald* here proposes are, as to their effect, noways inferior to the wooden bellows, made use of in *Sweden* at all their iron forges, and furnaces, &c. but far more advantageous in all other smelting works, that require large bellows.

Whoever has seen the invention, described in the *Philosophical Transactions*, and made use of at *Tivoli* in *Italy*, and several other places, and call'd *soffi d'acqua*; and attentively considers the following description, will be convinced, that this new invention of water-bellows is built on the very same foundation, to which leathern and wooden bellows owe their use and original, and in several cases prove of more signal service.

The water-bellows A and A (as represented in the Fig. Plate XII.) are made of wood, not unlike the shape of diving bells, in the form of a truncated cone, and consequently wider below than at top, where they are furnish'd with close heads B and B, but at the lower ends E and E quite open. At the heads B and B are two valves V and V, which open internally, and are made like the claps of other bellows, with their hinges, and the valves themselves cover'd with hatters felt, and shut by an easy steel spring, till the air from above opens the same, which happens only when these bellows receive their motion upwards; but are shut by means of the pressure of the air within, when they

Fig. II.

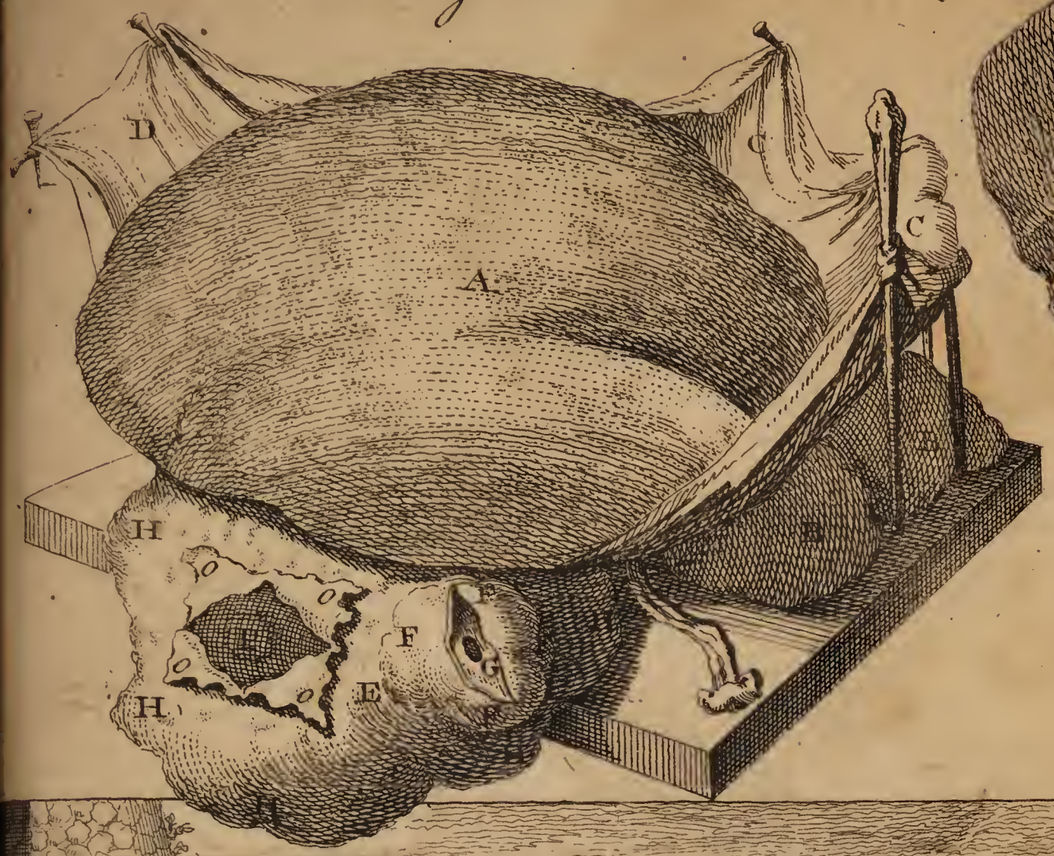


Fig. III.

Fig. IV.

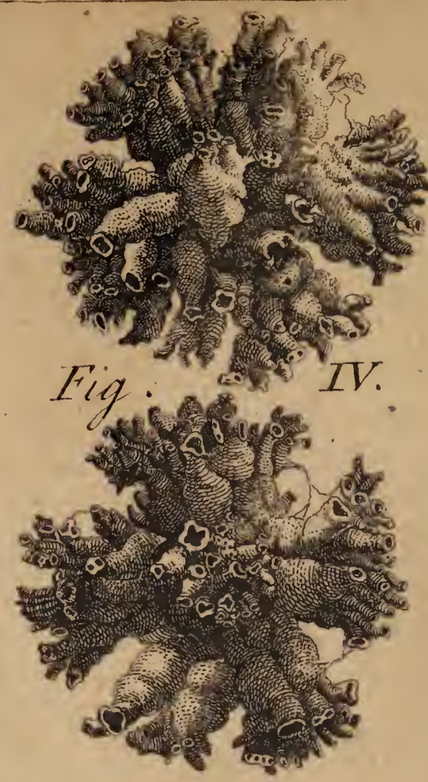


Fig. VI.

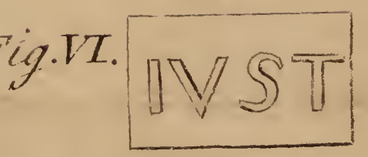


Fig. I.

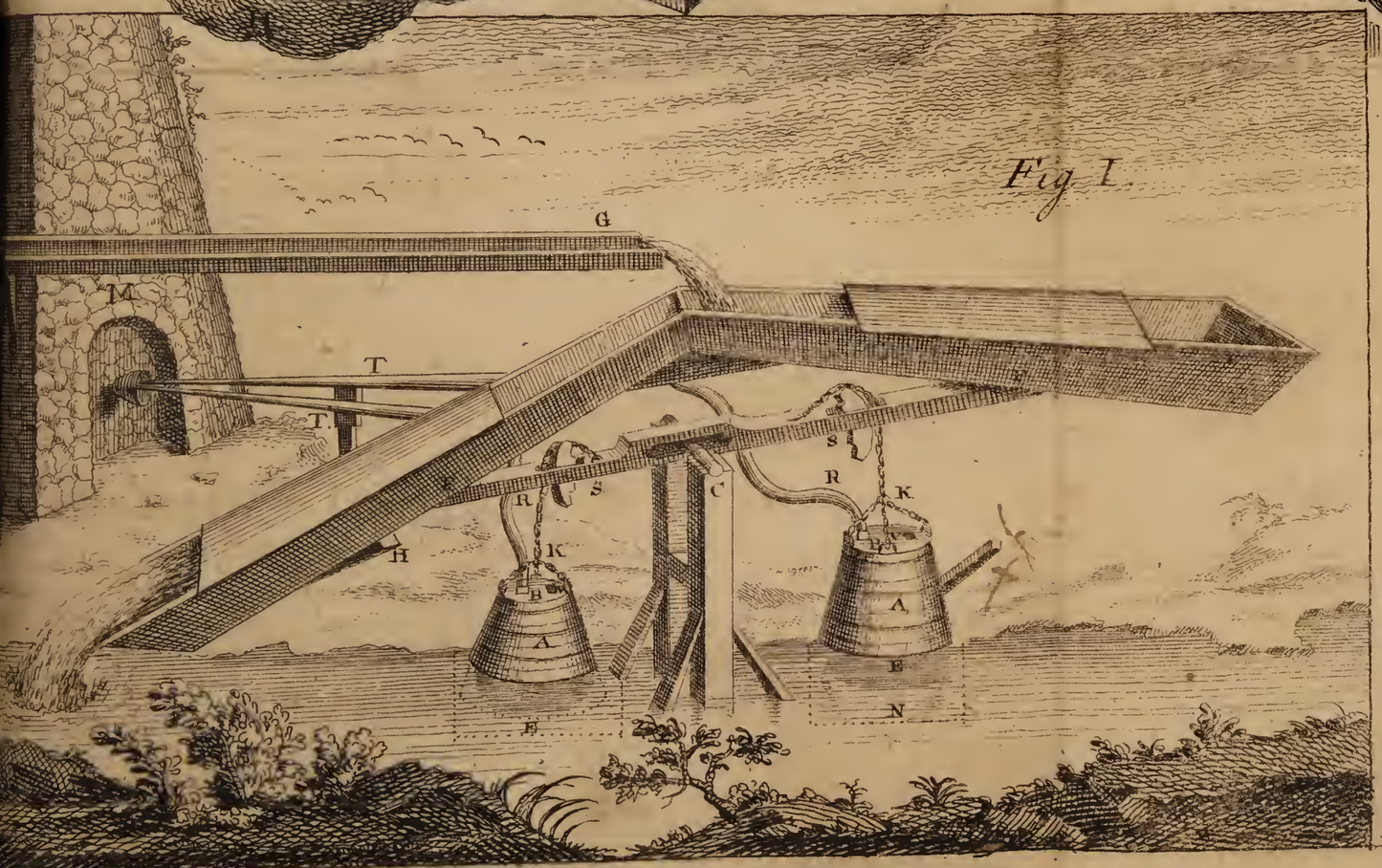


Fig. VII.

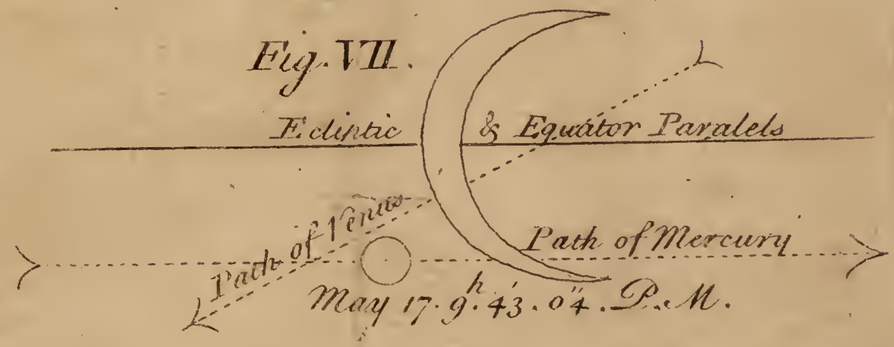
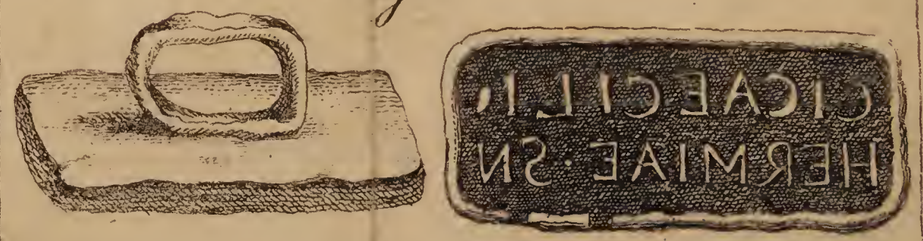


Fig. V.



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J. Hulett Sculp.

they ſink down into the water. On the very ſame heads are two pliable leathern tubes R and R, fixt one at the top of each water-bellows, which tubes are made and prepared in the ſame manner as thoſe uſed in water-engines for extinguiſhing fire: Theſe leathern tubes or pipes reach from the bellows to wooden tubes, T, T, which carry the wind into the iron furnace M, or any other place, according to pleaſure.

Theſe bellows are likewiſe provided with iron chains K, K, which are faſtened to two ſweeps, S, S, by which means they hang perpendicular from the beam of the balance, and at the ſame diſtance from the centre of its motion C.

On the balance are two ſloping gutters, F, F, into which the water alternately runs from the gutter G, and ſo gives motion to the whole work: So that theſe laſt mentioned gutters do the ſame ſervice as an over-ſhot, or any other water-wheel, and coſt a great deal leſs, but give as even and regular a motion, as any pendulum for meaſuring of time: For, as ſoon as ſo much water runs into either of the afore-mentioned inclined planes of the gutters, ſo that the momentum of the water exceeds the friction near the centre of motion C, the gutter immediately moves down, with a velocity increaſing, till the balance meets with the reſiſtance of the wooden ſprings H and H, and at the ſame time raiſes the oppoſite water-bellows, or that bellows which is fixt under the oppoſite gutter. In the ſame moment again, as the ſaid gutter begins its motion, being come down on the ſpring, delivers all the water it has receiv'd; and the very ſame time the water begins to run into the oppoſite gutter, which receives its load of water almoſt as ſoon as the former is emptied: [So that one of the gutters does its effect, as ſoon as the other has done its effect; and this alternately one after another.]

Theſe ſloping gutters on the balance do, therefore, all the ſervice and effect, which a water-wheel does in working the ordinary bellows, and that by means of the power which the water applies to the wheel for giving the common bellows their motion; after the ſame manner does the water here enable the ſloping gutters to do the ſame work.

But as to the manner and the means whereby theſe water-bellows are fit to blow the fire, and perform the ſame as leathern and wooden ones, there is no other reaſon but the very ſelf-ſame wherein the effect of the ordinary bellows conſiſts: For, an ordinary pair of bellows blow for no other reaſon, but that

the

the air which enters the bellows, and which they contain when rais'd, is again compreſs'd or forced into a narrower ſpace, when the bellows cloſe: Now ſince the air, like all other fluids, moves to that place where it meets with the leaſt reſiſtence, it muſt conſequently go thro' the opening left for it, with a velocity proportioned to the force by which the air is compreſs'd, and muſt neceſſarily blow ſtronger or weaker, in proportion to the velocity by which the top and bottom of the bellows meet; the blaſt will alſo laſt in proportion to the quantity of air, that was drawn into the bellows thro' the valve or wind clap.

This happens in the ſame manner in water-bellows: For, the air which they contain cannot force itſelf down through the water more than thro' a well ſecured deal board with pitch; when the bellows are lower'd down into the water, the air which they contain muſt neceſſarily be compreſs'd by the water which riſes alternately into the bellows A and A: So that the air muſt recede and go thro' the leathern tubes R, R, where the air meets with the leaſt reſiſtance.

From all which it undoubtedly follows, that the larger, that is to ſay, the more air theſe water-bellows are made to contain, and the greater the velocity is by which they are made to deſcend into the water, ſo much the greater is their effect; and that the effect, which they are able to perform, muſt be equal to that of leathern or wooden bellows of the ſame capacity, in containing an equal quantity of air.

As to the advantages which this new invention has in regard to theſe hitherto made uſe of, it is a thing known, that the power which works the common bellows, made uſe of at iron furnaces, muſt be ſufficient not only to compreſs the bellows, but at the ſame time to force down the leaver with its weight or counterpoiſe; which leaver ſerves again to raiſe the bellows, when the cog or button on the axle-tree of the water wheel ſlides off from the bellows-tree: So that the power muſt be ſufficient at once to produce two different effects; whereas theſe new water-bellows require ſcarce any greater power than what is neceſſary to overcome the friction near the centre of motion, or the axis C: For, in this invention of M. *Triewald*'s an advantage is obtain'd, which very rarely happens in mechanics, *viz.* that the weight to be mov'd is, as here, on the balance in *æquilibrio*; ſince the bellows A and A cannot be otherwiſe conceiv'd than as two equal, tho' heavy, weights in a pair of ſcales, which balance each other, tho' their weight be ever ſo great: So that if each

of

of these bellows should weigh a ton, they must still equiponderate; which is so much the easier attain'd to, since it requires very little art to make them both of a weight, and order them at equal distances from the centre of motion.

It is, consequently, known how small a power is required to set the scales of a balance with equal weights in motion; notwithstanding the weight may be as great as possible: All which may with good reason be applied to these water-bellows.

And tho' it cannot be denied, but that the bellows which sinks down into the water-hole or sump N, becomes so much lighter, as it loses of its weight in water, by which means the water-bellows to be rais'd becomes so much heavier, as the former loses of its weight by being let down into the water; yet this is compensated, if we consider that the water which falls down along the sloping gutter, acquires a power of a falling body; which power increasing in the same proportion as the bellows to be rais'd grows heavier, this power suits admirably well the weight to be rais'd: For, the bellows that sinks down into the sump N, does not at once lose its weight in the water, but gradually as it comes deeper into the same: And in the same manner the ascending bellows does not grow at once heavier than the other, but gradually, growing heaviest just when the lowermost edge gets even with the surface of the water; and that happens at the same instant of time when the power of the water in the sloping gutter is at the highest pitch, or has receiv'd its greatest *momentum*.

This shews very plainly, that the power requir'd to work these water-bellows is far less; and consequently, less water will be consumed in working these bellows than those commonly used: And again, that an iron furnace, which for want of water to work the common bellows, cannot be kept at work longer than six weeks, tho' it be provided with all necessaries, may by means of such water-bellows as here described, be kept at work at least as long again.

It is furthermore a thing known to miners, of what prodigious loss and inconveniency it is, when the hearth or mouth of an iron furnace is placed low, in a wet and damp place, which they are oftentimes obliged to do, in regard to the axle-tree of the water wheel, which works the bellows; for which reason such furnaces as stand in the like moist

places

places give daily conſiderably leſs iron than others which are better ſituated.

There is likewiſe no ſmall difficulty to find a fit ſituation for ſuch iron furnaces where iron guns are caſt, and require deep pits under the mouth of the furnace: But by means of this new invention of bellows, one may be at liberty to place the mouth of the furnace as high as one pleaſes, ſeeing it is very eaſy to guide the blaſt by means of wooden or leaden tubes, as far as neceſſary, and in a proper direction into the furnace; which advantage cannot ſo eaſily be obtain'd by thoſe bellows in common uſe.

Further, this may be accounted as no ſmall advantage which theſe bellows afford, in being of ſo very eaſy a ſtructure, that any carpenter at firſt ſight is able not only to conſtruct the whole engine, but eaſily repair every part of the ſame; requiring at the ſame time the leaſt repairs of any that can be uſed: And if the bellows ſhould be caſt iron, they would laſt for ſeveral ages; and when caſt ſtrong, they would not require any weight to ſink readily in the water. One might cauſe them to be cover'd with lead, or make them of thin copper with a thick leaden hoop at top, to make them ſink. As to their ſhape, it is not abſolutely neceſſary that they ſhould be of the ſame with that repreſented by the Fig. For, in caſe one would not beſtow iron hoops on the bellows, they might be made ſquare, in a triangle, or any other ſhape, provided they be as wide again at bottom as at top: And if they be made of wood, it will be neceſſary to provide an edge round the tops, for containing as much ſtones or leaden weights, as will be found neceſſary to make them ſink readily, when they are lower'd down into the water.

Laſtly, if we will conſider the charge of thoſe bellows made uſe of at iron furnaces, as to the bellows themſelves, the water-wheel and its axle-tree, &c. and compare the ſame with the coſt of theſe, we ſhall eaſily find a vaſt difference, not to mention the vaſt charges of keeping the common bellows in repair.

In fine, the blaſt of theſe bellows is governed and moderated in the ſame manner as the common ones, viz. by letting more or leſs water into the ſloping gutters, and by taking out and letting in plugs for that purpoſe placed in holes near the top of the water-bellows.

An Abstract of Meteorological Observations for six Years, made at Padua ; *by* S. Poleni. Phil. Trans. N°. 448. p. 239. *Translated from the* Latin.

THE following is an abstract of six years meteorological observations, which were made according to the rules of Dr. *Jurin*'s invitation ; and consequently corresponding with those published in *Phil. Trans.* N° 421. The instruments *S. Poleni* made use of were the same, and posited in the same places, and applied in the same manner, as mentioned in that transaction. The observations are, as follows :

TABLE A.

	1731 In. Dec.	1732 In. Dec.	1733 In. Dec.	1734 In. Dec.	1735 In. Dec.	1726 In. Dec.
January	2 546	2 129	1 855	1 034	4 052	6 541
February	3 093	1 959	0 405	1 735	2 420	2 981
March	0 976	2 765	5 642	1 558	5 162	2 721
April	3 434	5 432	3 816	1 706	1 452	1 227
May	0 602	1 864	5 330	4 372	2 681	4 444
June	4 253	2 872	2 712	4 555	3 865	2 777
July	3 402	1 585	3 874	7 015	4 992	3 064
August	7 372	3 112	3 679	3 082	0 720	1 844
September	2 216	0 089	0 589	2 899	1 287	2 479
October	4 354	9 164	2 788	4 391	1 878	0 529
November	1 653	0 957	0 382	1 307	0 542	1 454
December	0 306	3 528	1 065	4 909	0 634	0 572
Sum of the whole year.	34 207	35 456	32 137	38 563	29 685	30 633

And first, the quantities of rain and snow water, collected for these six years, are exhibited in Table A : And if all the months of *January*, *February*, &c. of the said years, be added together, it will be found that the least quantity of water, namely 6 inches 295 dec. fell in the months of *November* ; on the contrary, that the greatest quantity, *viz.* 23 inches 932 dec. fell in the months of *July*. Whereas in the six preceeding years, the least quantity fell in the months of *February* ; and the greatest in the months of *October*, and the difference between the greatest and least quantity was 22 inches 796 dec. but in this 17 inches 637 dec.

It likewise appears from the table, that the year 1735 was drier than any of the rest; the sum for that whole year being

29 inc. 685 dec. and the year 1734 wetter, its ſum being 38 inches 563 dec. In the ſix preceeding years the difference between the drieſt and wetteſt years was 27 inches 505 dec. in the ſix years of this table 8 inches 878 dec.

TABLE B.

	Winter. In.	Dec.	Spring. In.	Dec.	Summer. In.	Dec.	Autumn In.	Dec.
1731	5	759	6	647	13	598	8	17
1732	4	522	10	300	7	226	10	186
1733	6	321	15	758	8	762	3	759
1734	4	74	8	14	14	34	10	125
1735	10	450	10	848	7	805	2	337
1736	11	945	8	54	6	361	4	588
Sum.	43	71	59	621	57	796	40	12

In this table B it eaſily appears, that the quantity of water gather'd in ſummer and autumn for three years was greater than that gather'd in winter and ſpring; but that for three years it was leſs: Whereas in the ſix preceeding years the quantity in ſummer and autumn was always greater; and in theſe ſix years the ſeaſons were to be ranged according to the increaſe of the ſums of water gather'd in the following order, winter, ſpring, ſummer, autumn: In the ſix years of Table B they ſhould be ranged thus, autumn, winter, ſummer, ſpring.

In theſe laſt ſix years, the ſum of the quantity of water collected in ſummer and ſpring exceeds that collected in winter and autumn. And in both the ſix years, ſummer is refer'd to the two ſeaſons of the greateſt, and winter to the two ſeaſons of the leaſt quantity.

TABLE C.

The decreaſe of the barometer from the noon of the preceeding day to that on which it rain'd		The increaſe of the barometer from the noon of the preceeding day to that in which it rain'd.	
Number of days in which it rained.	The wind what at the noon of the days it rain'd.	Number of days in which it rain'd	The wind what at noon of the days it rain'd.
140	N	80	N
47	N E	29	N E
15	E	7	E
18	S E	4	S E
27	S	14	S
28	S W	17	S W
33	W	24	W
62	N W	31	N W
Sum 380		Sum 206	

As in the correſponding table for the former ſix years; ſo in this, there was no greater difference between the numbers of increaſe and decreaſe of the height of the barometer in rainy days, than what is between the numbers 370 and 206; which is almoſt the ſame with that for the former ſix years, namely, 378 and 211.

It is likewiſe worth remarking, that in all the former ſix years, there were 589 rainy days; and in the latter 576, there being only the ſmall difference of 13 days between both the ſix years; and likewiſe in both the ſix years there was a greater quantity of rain with a northerly than with any other wind; and the leaſt quantity with a S E and E. Moreover, the ſame things are to be underſtood in the above Table C, as in that in *Phil. Tranſ.* N° 421.

Table D

The decreaſe of the barometer from the noon of the preceding day to that of the day it ſnow'd.		The increaſe of the barometer from the noon of the preceding day to that of the day it ſnow'd.	
Number of days it ſnow'd.	The wind what at the noon of the days it ſnow'd.	Number of days it ſnow'd.	The wind what at the noon of the days it ſnow'd.
3	N	5	N
1	NE	2	NE
1	W	1	SW
1	NW	1	W
Sum 6		Sum 9	

In the former ſix years it ſnow'd in the decreaſe of the barometer for more days than in the increaſe: But the contrary may be ſeen in the above table D. In the former ſix years there were 18 ſnowy days, and in the latter ſix, 15.

Table E

	The ſum of the heights of the barometer.		The ſum of the heights of the thermometer.		The mean height of the barometer for each day.		The mean height of the thermometer for each day.	
	Inches	Dec.	Inches	Dec.	Inches	Dec.	Inches	Dec.
1731	10850	65	18286	25	29	72	50	9
1732	10870	19	18361	30	29	70	50	17
1733	10867	18	18301	95	29	77	50	14
1734	10850	24	18305	78	29	73	50	15
1735	10861	21	18274	87	29	76	50	6
1736	10870	7	18338	42	29	70	50	10

In the table E may be ſeen the ſums of the heights of the barometer and thermometer for each year; as alſo the mean heights correſponding to each day.

For the whole laſt ſix years, the mean height of the barometer, referable to each day of the ſaid ſix years, is 29 inches 73 dec. differing only, 03 parts from that of the former ſix years, which was 29 inches 70 dec.

And likewiſe the mean height of the thermometer for each day of theſe laſt ſix years is found to be 50 inches 12 dec. differing only ,04 parts from that of the former ſix years, which was 50 inches 16 dec.

In general, the mean heights both of the barometer and thermometer for each day in each year differ but little in this table; as they likewise very much agreed in the table of the former six years.

Table F

Years	Months	Day O. S.	Hour h.	Greateſt height of the barometer. Inc.	De.	Leaſt height of the barom. In.	Dec.	The height of the thermo. In.	De.	Winds	Weather.
1731	Feb.	6	15	30	26			48	36	N W	Fair
	Jan.	29	15			28	70	47	92	S E	The air dark.
	Dec.	10	15	30	20			48	32	N	Fair.
1732	Mar.	11	2			28	85	49	67	S W	Sky overcaſt.
	Jan.	23	15	30	48			48	62	N W	Fair.
1733	Mar.	19	15			28	96	49	18	N	Rain.
	Jan.	12	15	30	34			48		N	Fair.
1734	Dec.	15	15			28	88	48	30	S	Sky overcaſt.
	Feb.	8	15	30	30			48	40	N W	Sky almoſt overcaſt.
1735	Mar.	17	15			29		49	48	W	Small rain.
	Nov.	19	15	30	20			48	70	N	Fair.
1736	Feb.	12	15			28	88	48	74	N W	Sky overcaſt

Years

Table G

Years	Months	Day	Hour h ′	Height of the barometer. Inc.	Dec.	Greateſt height of the thermometer. Inc.	Dec.	Leaſt height of the thermometer. Inc.	Dec.	Winds	Weather.
1731	Jun.	29	15	29	80	52	40			SE	Fair.
	Jan.	27	15	29	30			47	44	NW	Small rain.
1732	Jul.	20	4	29	62	52	52			NW	Sunſhine and clouds alternately.
	Dec.	6	15	29	55			47	75	W	Fair.
1733	Jun.	29	15	29	86	52	38			NW	Fair.
	Dec.	13	15	30	10			47	85	W	Sky almoſt overcaſt.
1734	Jul.	1	15	29	70	52	24			NE	Sunſhine and clouds alternately.
	Jan.	14	15	30	12			47	92	SW	Fair.
1735	Aug.	26	15	29	78	52	18			W	Fair.
	Dec.	27	15	30	14			47	74	W	Sunſhine and foggy.
1736	Jul.	22	15	29	90	52	30			NE	Fair.
	Dec.	20	15	29	78			47	92	W	Sky overcaſt.

S. Polus

S. *Poleni* has added the tables F and G, in which are ſhewn the greateſt and leaſt heights both of the barometer and thermometer. By comparing them with the correſponding tables of *Phil. Tranſ.* N° 421 we may ſee, that the greateſt height of the barometer, namely 30 inc. 48 dec. exceeds that in the former ſix years, which was 30 inc. 40 dec. But the falling of the mercury to 28 inc. 70 dec. is leſs than that in the former ſix years, which was obſerv'd to be 28 inc. 56 dec.

In theſe latter tables the greateſt height of the thermometer is 52 inc. 52 dec. and in the former 52 inc. 54 dec. the difference being ,02 only: But the leaſt height in theſe tables is 47 inc. 44 dec. in thoſe 47 inc. 58 dec. The mercury, therefore, fell in the latter ſix years lower by 14 dec. than it had ever done in the former ſix years.

Table H.

Years N. S.	Inc. of a *Paris* foot.	Lines
1731	31	11 $\frac{3}{5}$
1732	30	$\frac{1}{2}$
1733	32	5 $\frac{1}{2}$
1734	35	5 $\frac{1}{2}$
1735	28	7 $\frac{4}{5}$
1736	29	2 $\frac{4}{5}$
	Sum 187	9 $\frac{1}{3}$

By dividing into ſix equal parts 187 inc. 9 $\frac{1}{3}$ lin. (the numbers got from the ſum of this laſt table H) the mean meaſure of water for each year is 31 inc. 3 $\frac{7}{12}$ lin. But in the former ſix years it was found to be 35 inc. $\frac{7}{12}$ lin. The difference, therefore, is 3 inc. 9 lin.

And if we bring the ſums of both the ſix years into one ſum, and divide it by 12, we ſhall find 33 inc. 2 $\frac{1}{2}$ lin. to anſwer to each year: And if this quantity (as has been done in the former ſix years) be compar'd with the mean quantity of water, that falls at *Paris*, namely 19 inc. or 18 inc. 8 lin. it will ſtill plainly appear, that a much greater quantity of water falls at *Padua*, than at *Paris*.

And theſe ſix years do likewiſe furniſh with an obſervation, by which it appears, that ſometimes in 24 hours there falls a much greater quantity of water at *Padua* than ever does at *Paris*

Paris in the ſame time. From the noon of the 27th of *Oct.* 1732, O. S. the wind at north, till the noon of the following day, there fell 2 inc. and 9 lin. of rain nearly. Moreover, the exceſs of this quantity above that (within the time already ſpecified) which falls at *Paris* may be eaſily known by comparing the *Memoirs of the Royal Academy of Sciences*, as S. *Poleni* has hinted elſewhere.

The greateſt height of the barometer on *Jan.* 23, 1733, obſerv'd theſe laſt ſix years, and reduced to *Paris* meaſure, is 28 inc. 6 $\frac{9}{10}$ lin. and the leaſt height anſwering to the 29th of *Jan.* 1731 is 26 inc. 10 $\frac{9}{10}$ lin. The difference between the greateſt and leaſt height, is found to be 1 inc. 8 lin.

And ſince the difference (as has been mention'd in *Phil. Tranſ.* N° 421) between the greateſt and leaſt height of the barometer at *Paris* is found to be 1 inc. 11 $\frac{1}{6}$ lin. it exceeds that found by S. *Poleni* at *Padua* by 3 $\frac{1}{6}$ lin. Wherefore, what has been obſerv'd of theſe differences in the ſaid *Tranſaction* is confirmed by theſe later obſervations.

In fine, S. *Poleni* adds ſome obſervations on the declination of the magnetic needle. In *April*, 1733 he found by repeated obſervations that the declination was 13 degrees and $\frac{1}{2}$ weſtwards. On the laſt days of the year 1736 he found it 13 degrees and 45 ſexageſimals. If, therefore, what has been already ſaid of the declination of the needle in *Phil. Tranſ.* N° 421, be compar'd with this, it will plainly appear, that the declination for the 3 firſt of theſe ſix years increas'd more than it did for the three laſt.

The imperfections of the common Barometers, *and the improvement made therein*; *by Mr.* Charles Orme; *together with ſome Remarks*; *by Mr.* Henry Beighton. Phil. Tranſ. N° 448. p. 248.

AS there is nothing more wanting than a theory of the weather on mechanical principles; there does not ſeem any thing in all philoſophy of more immediate concern than the ſtate of the weather.

In order to which a complete hiſtory of the weather is neceſſary, to deduce from thence ſuch rules, and obſervations, as may in ſome meaſure form ſuch a theory: And it may be ſaid, that could we in any tolerable degree foretell but by ſome ſmall ſpace of time, the change of the weather, it would be of admirable uſe to us in thoſe affairs on which the chief part of our welfare and ſubſiſtence depends.

It

It was from ſuch conſiderations, that upwards of 20 years before Mr. *Orme* began, and had continu'd to keep a diary of the weather, (the ſix laſt years of which he here ſubjoins) and tho' he pretends not to form a juſt theory upon them, yet he is not without hopes they may have their uſes, when they fall into abler hands.

From them, and the obſervations he made by a new improvement of the barometer (for the ſame number of years) he could generally foretell for a day, or perhaps two, the change of weather, or its continuance,

And tho' ſo many ingenious and curious perſons, ſince the invention of *Torricelli*'s barometer, have been improving and endeavouring to bring that machine to perfection; yet notwithſtanding all their indefatigable care and pains, the air that is interſperſed and mixed with all fluids (of which mercury is reckon'd one) has in ſome meaſure fruſtrated their labours, and it has remained imperfect: For, whilſt there are any ſmall quantities or particles of air remaining in the quickſilver, it will be conſtantly riſing in hot weather, and falling in cold; which really perverts the very end and deſign of a barometer, which ſhould ſhew the preſſure of the air, and foretel when either fair weather or rain is coming: Inſtead of which it is in great meaſure a thermometer, foretelling heat inſtead of fair, and cold inſtead of rain and ſtormy weather: And theſe imperfections are more or leſs in all the various ſorts of barometers hitherto invented.

The barometer Mr. *Orme* is going to deſcribe is not different in form from ſome uſually made, it being of the diagonal kind, from whence the more minute alterations are more readily diſcover'd: Of this form ſeveral have been made by the late ingenious Operator Mr. *Patrick*, who has, in his way, deſerved well of the curious: And who, tho' he had done ſo much towards proving the weight of the atmoſphere, by which the mercury in the tube was ſuſtain'd, did himſelf not believe it, but run into the abſurdity of the funicular hypotheſis.

There is an inconveniency or imperfection in moſt, if not all, thoſe diagonal barometers: For after ſome time, the various riſing and falling and changes of the weather, of heat and cold, the ſmall particles of air that have been interſperſt in the mercury, have got together in a larger maſs, as they will incline by attraction, which will ſeparate the mercury; and that quantity of air will be dilated by heat, and contracted by cold; ſo as to ſpoil the deſign thereof.

Beſides, there is ſuch a coheſion of the mercury to the tube (eſpecially in the ſmall ones) that after ſome time, the mercury that is not truly cleans'd from its droſs, and purg'd of all its air, will in remarkable changes of the weather, neither riſe nor fall: All which embarraſſment is taken off, and the difficulties ſurmounted in Mr. *Orme*'s improvements of the barometer, by the following method.

I. The quickſilver is entirely purified from its droſs and earthy particles by diſtillation; and when the tube is fill'd with a pound and a half, two or three pounds of mercury, and all the air got out by the uſual methods in filling tubes, then the remaining air is diſcharged by ſuch an intenſe degree of fire, as makes the mercury boil; by which ebullition an innumerable quantity of ſmall particles are emitted, and blow with a great velocity at the open end of the tube, till all the air be quite cleared out; which curious as well as laborious operation is continued for the ſpace of four hours: And when no more bubbles would riſe in the tube, it remain'd whole, with its mercury of a very lively ſparkling brightneſs, with this difference only, that the mercury, ſo purged of its air, did not fill the tube ſo high, as when firſt put in by about two inches; which is a plain demonſtration, that in that tube, which was 49 inches long, there was interſperſt in the mercury at firſt filling it, ſo much air as would fill two inches of the ſaid tube, which was a twenty fourth part of the ſaid ſpace.

The whole operation Mr. *Orme* himſelf attended the 20th of *January*, 1734-5.

And farther, he can affirm, that every part of the mercury boil'd for a long time, and the tube became gradually ſo red hot, that with a warm knife he could make impreſſions in any part of it.

And that this is the ſureſt, if not the only, method for getting out all the air, may be judged by the boiling of water, which in its ebullition emits a great quantity of air for a conſiderable ſpace of time.

As to the perfection of theſe barometers, they exceed all others Mr. *Orme* ever obſerv'd in the following particulars.

1. They are ſenſible of the moſt minute changes of the air.

2. They foretel the weather by a much longer ſpace of time than others, as for the moſt part 20 hours, ſometimes 30 or 48 hours: Nay, before great ſtorms, and ſuch rains as cauſe great floods, for a much longer time before they happen.

3. Tho' they are ſo ſenſible of ſuch minute changes of the air

air, yet the moſt intenſe heat will not raiſe them a hair's breadth, nor the greateſt cold make them fall. This ſhews they are perfect barometers, and not in any degree thermometers.

4. You may diſtinguiſh by them, whether (if they ſhew for rain) it will be little or much.

5. As by other barometers you cannot tell the weather, but by a paſt and a preſent obſervation; theſe tell you the inſtant you come to them, what the weather is going to be: For, by rapping the caſe with your finger, if it is going to be fair, or very fair weather, the mercury will riſe that moment, a 10th of an inch or more; but if for foul, it will ſcarce make any ſenſible riſe.

Mr. *Orme* has had one of the glaſſes by him for 10 years, and he has conſtantly obſerved its motions, which has very ſeldom fail'd him in foretelling any conſiderable change of the weather.

Mr. *Beighton* makes the following remarks

1. Tho' you can foretel it will rain on the morrow, it is impoſſible to tell where the rain will fall: For, as every ſhower has ſpace, i. e. length and breadth, if it rains in that particular field, yet it may be fair in the next adjoining.

2. The barometer does only ſhew the preſſure of the atmoſphere, and inclination of the air, in and about the country where it ſtands, and not always in a particular ſpot: So that in foretelling of great rains, tho' they be over us when the glaſs fell; yet the wind which bloweth where it liſteth, carries the clouds and rain with it.

3. On the mercury's falling it is very hard to diſtinguiſh, whether it will be rain or high winds, they equally cauſing the mercury to ſubſide.

4. Of all who gueſs at the weather, it is obſervable it is not true one time in ten, nor do any two of them agree about it.

But from obſervations on this barometer, it will ſeldom fail you once in 20: So that it is above 100 to 1 preferable.

5. If from the ſtate of the mercury, for inſtance, yeſterday and this morning, it be pronounced, next day will be no rain, and one look at the glaſs no more that day, perhaps winds may ariſe, and ſo alter the weight of the atmoſphere, and the glaſs falls conſiderably, it will rain on the morrow, contrary to what was at firſt expected.

Hence it is evident from theſe remarks, that judgments are formed on the weather from barometers, which do not

prove ſo; and this begets an opinion in the vulgar and ignorant, that there is no judgment at all to be formed from them.

If the barometer could only foretel very great and remarkable changes of the weather; for inſtance, in harveſt time, that a very great rain, or perhaps floods, were coming, the husbandman would ſtop cutting down his grain, and ſave ſome of it being ſpoil'd by the wet. Or on a journey, if a perſon knows from the indications of the barometer, that if he does not get home by ſuch a time, or paſs ſuch rivers, the floods will be ſo great as not only to prevent him but endanger his life, and this muſt be reckoned of conſiderable ſervice.

The greateſt ſtorm that has been in our days, was *Jan.* 8, 1734-5. On the 5th the mercury began to fall, and on the 8th it was a 10th below 28 inches; which has not been obſerved in this age, nor perhaps ſince *Torricelli*'s time: Thence one could plainly indicate, that it would be the greateſt flood ever heard of, or the greateſt ſtorm ever felt; the latter of which it prov'd.

Some rules and obſervations for foretelling the weather by the riſing and falling of the mercury.

Tho' riſing always preſages fair, and falling, foul weather; yet there are ſeveral difficulties and niceties in forming a judgment from them, and herein conſiſts the chief art.

Moſt of the obſervations, made by Dr. *Halley*, Dr. *Beal*, Dr. *Derham*, Mr. *Patrick*, and others, are applicable to this improv'd diagonal barometer.

Mr. *Beighton* inſerts here ſome few obſervations for the improved diagonal barometer, which he believes may be reckon'd rules as he has deduced them from time to time, in uſing Mr. *Orme*'s glaſſes, and keeping a regiſter of the weather.

1. This barometer very rarely foretells thunder, ſeldom falling at all before it, which Mr. *Patrick* obſerves others do.

2. In ſerene and hot weather, when the mercury is high and riſing, and there is all the poſſible certainty of fair weather the next day, and if there happen to fall great ſhowers, you may conclude they have been driven upon you by thunder, tho' you have heard nothing of it.

3. When the mercury is pretty high, and has fallen to foretel rain, and it riſes again before the rain cometh; it indicates that there will be but little of it.

4. If the mercury continues falling whilſt it rains, it ſhews it will rain next day.

5. In

5 In fair weather when the mercury has continued high or riſing, if it falls a little to day about noon, and towards the evening riſes again, you muſt expect a ſingle ſhower the latter part of the next day (or perhaps by noon) and then fair weather again forward.

6. When the mercury riſes gradually about half a 10th perpendicular height, and continues ſo to do for many days together; you may reaſonably expect a fair ſeaſon for as long a time as it was riſing, unleſs ſome gales of wind intervene eſpecially the S W b S, or thereabouts.

7. When the mercury riſes or falls very faſt, neither the fair or foul weather it forbodes will continue long.

8. Without knowing how the mercury has ſtood ſome little time before, a true judgment cannot be formed at all times: For ſuppoſe, I find it in a riſing condition, I am apt to think it will be fair; but if it had been higher ſome hours before, and fell, there muſt happen a ſhower.

Why the mercury in the diagonal barometer (if it be for fair weather) on rapping the caſe ſeveral times, which jars and makes the tube tremble, will riſe at every ſtroke for ſeveral ſtrokes together, and in all ſometimes a tenth of an inch or more in the perpendicular, may, Mr. *Beighton* preſumes, be thus accounted for.

1. There is a coheſion of the mercury to the tube, which hinders its riſing, and ſuch rapping releaſes that.

2. But its obſervable, that it will riſe a little at all times, even when it is in a ſtanding or even in a falling condition: This may be accounted for thus

The mercury and atmoſphere are in *æquilibrio*; and rapping ſtarts and raiſes the mercury a little in a boiling manner, eſpecially its upper ſurface, which is obſerv'd to leap, or be in a ſwimming poſture; then the preſſure of the atmoſphere overbalances the remainder of the mercury, and it muſt riſe a little.

Or ſuch violent jarring puts the mercury in a lateral and upward motion (for, downward it cannot go) which takes off its gravity, as the winds leſſen the preſſure of the air; therefore, it muſt riſe a little.

But then it is alſo obſervable, that if the mercury was in a ſtanding or falling condition; ſuch riſing as above will in a minute come to the ſame place again; and even when the mercury is in a riſing condition it will in that ſpace of time fall a little part of what it roſe by ſuch rapping.

This barometer has the coruſcations, as they were obſerv'd in Mr.

Mr. *Patrick*'s pendant one: For, by rapping the caſe with the finger in a dark place, it will emit ſeveral bright flaſhes, along the empty part of the tube.

This Mr. *Beighton* takes to be an argument that the vacuum is very pure; and the mercury truly purged.

Mr. *Beighton* adds, that he can with truth affirm, that in all the affairs of husbandry, but eſpecially in harveſt, it has ſeldom fail'd him in foretelling the weather ſo well, as to be very advantageous to him for 9 years together, and ſo pleaſing and entertaining at other times, that he could not be without one of theſe barometers.

The following are Collections from the diary of the weather and barometer, in order to ſettle rules for foretelling the weather from the latter.

Great STORMS.

Before them the mercury falls 3 or 4 days, and is exceeding low

	1734-5.					
Jan.	4	at night the mercury at	29.	92.	inches.	
	5	night	29.	66.		
	6	night	29.	2.		
	7	night	28.	1.		
	8	noon	27.	9.	lower by $\frac{1}{10}$. than	

has ever been known, and the greateſt ſtorm of wind ever heard of in this age, in the ſouth of *England*; as alſo in *France* and *Holland*.

	1736.				
Jan.	31.	29	47		
Feb.	1.	29	15		
	2.	28	39	rain and ſtormy.	
	1734.				
Aug.	11.			ſtormy.	

Great FLOODS.

Before which the mercury falls very conſiderably

	1735.			
Sep.	4.	29	7	
	5.	29	6	
	6. night	29	6	
	7.	29	25	the greateſt flood

(at *Coventry* being about the middle of *England*) that has been theſe forty years, and yet the mercury fell but little.

	1735.			
Oct.	23.	29	55	
	24. night	28	8	
	25. night	28	78	
	26.	28	85	
	27.	28	26	great flood.
	1735.			
Aug.	19.	29	3	
	20.	29	28	
	21.	29	3	
	22.	29	2	
	23.	29	2	ſtormy, great rain.
	24.	29	38	floods.
Dec.	2.	29	32	rain.
	3.	29	5	fair.
	4.	28	8	rain.
	5.	28	9	rain.
	6.	29	5	fair.
	7.	29	52	great rains and floods.

THUNDER.

The mercury ſeldom falls for rains that come by thunder. *Vide Diary June* 2, 1735.

THUNDER.

When the mercury did riſe.

	1733.						
June.	21.	—	29 16	—	29	56	
	22.	—	29 56	—	29	56	
	23.	—	29 62	—	29	65	hot.
	24.	—	29 65	—	29	57	ſultry.
	25.	—	29 54	—	29	52	ſultry.
	26.	—	29 51	—	29	59	great thunder.
	27.	—	29 57	—	29	56	a very violent thunder from 10 in the morning till one in the afternoon, doing a deal of damage.
	1735.						
June.	1.	—	29 3	—	29	8	
	2.	—	29 4	—	29	55	thunder and great rains.

THUNDER

The mercury fell before it. 1733.

1733					
July 27.	—	29	44		hot, fair.
28.	—	29	37		wind. rain.
29.	—	29	09		violent thunder.
1734 *Aug.* 7.		29	59		ſultry
8.		29	46		fair
9.	—	29	25		thunder
10.	—	28	87		rain, thunder

FROST.

A froſt, when the mercury is high, brings rain.

1731. The mercury was high all the month of *March*, and no rain, but what followed the froſt on the 17th and 29th.

Dry SEASON.

In *June* 1729, the mercury ſcarce ever above changeable.

In *Aug.* 1730. the mercury never lower than 29, 37.

1731. From the firſt to the tenth of *Aug.* and rain came the 16th, tho' the mercury was riſing.

FROST

A great froſt, tho' the mercury fell, but it was attended with a great ſnow, which might occaſion the mercury to fall.

1731							
Jan. 1.	—			—	29	56	rain
2.	—	29	46	—	29	12	rain
3.	—	28	78	—	28	72	wind
4.	=	28	72	—	28	81	froſt, great ſnow
5.	—	28	93	—	29	12	ſnow, froſt

Great RAINS.

Tho' the mercury was riſing.

1732							
May 1.	—	29	28	—	29	25	wind
2.	—	28	21	—	29	25	rain all day [ſnow hard from 8 to 11
3.	—	29	34	=	29	09	rain
4.	—	29	09	—	29	09	rain
5.	—	29	12	—	29	34	wind
6.	—	29	44	—	29	46	fair
7.	—	29	52	—	29	39	rain and great [floods

Great rain, tho' the mercury fell but little.

1733							
May 24.	—	29	6	—	29	54	wind
25.	—	29	51	—	29	54	fair
26.	—	29	52	—	29	54	fair
27.	—	29	5	—	29	39	violent rain for more than 11 hours

Great RAINS.

The mercury falling very much.

1734							
July 10.	—	29	65	—	29	67	fair, hot
11.	—	29	63	—	29	62	fair, hot
12.	—	29	59	—	29	4	rain
13.	—	29	29	—	29	13	great rains

—— The mercury falling a great while before the rain came, and the rain continued as long.

1736						
May 19.	—	29	75	fair, wind	29	8
20.	—	29	8	cold wind, fair	29	7
21.	—	29	65	cold wind	29	52
22.	—	29	39	wind, clouds, rain	29	52
23.	—	29	28	cloudy, fair	29	27
24.	—	29	32	fair	29	35
25.	—	29	32	cloudy, wind, rain	29	24
26.	—	29	15	rain	29	15
27.	—	29	12	rain	29	2
28.	—	29	28	rain	29	23
29.	—	29	37	wind, cloudy, rain		

1735				
Feb. 22.	—	29	43	
23.	—	28	82	
24.	—	28	9	great rain.
25.	—	28	76	

The mercury generally falls just after hot or sultry weather. *Vide* 16. *Sept.* 1731.

8. *Aug.* 1734.

There generally follow high winds after the *aurora borealis*.

1733

Oct. 27. A large *aurora borealis*; and the 28th, 29th, and 30th high winds.

Vide 23. *Jan.* 1734

The mercury falling pretty much, and neither wind nor rain ſucceeded.

1733
From the 18th to the 21ſt it fell 41 ; and no wind or rain at all till the 25th.

Sultry weather generally makes the mercury fall ſoon after.

1734 *Aug.* 8. After a great ſtorm the mercury riſes very faſt.
1736 *Feb.* 6.
1734 *Aug.* 26.

Before great winds the mercury falls very ſoon.

1736 *Feb.* 8.
1736 *Feb.* 8.

The mercury below 28 inches.

1734 *Dec.* 15, at 27 9
1735 *Jan.* 8. 27 9

In winter before froſt, the mercury riſes pretty faſt,

1735 *Dec.* 12.

Before a thaw the mercury falls.

1735 *Dec.* 13.
17.
1736 *Feb.* 9.

The mercury falls ſuddenly before a great ſnow.

1731 *Jan.* 4.
1736 *Feb.* 8.
Feb. 21.

When the mercury falls for high winds, and it continues to fall when that wind is come, it is likely to be tempeſtuous, or continue ſome time, unleſs rain ſucceed.

1736 *Nov.* 22.	—	29	62	fair, warm	29	62	
23.	—	29	49	windy, warm	29	32	wind
24	29	1	high wind		28	88	28 73 ſtormy.

Some of theſe collections are entirely contradictory to any ſettled rules ; and ſuch will happen, and others confirm them : But Mr. *Beighton* has collected ſo very few of a ſort, that till more are in this manner collected, it will be doubtful to form any rule from them.

Sulphureous Vapours from a cavern in a Stone quarry at Pyrmont ; *by Dr.* Seip. Phil. Tranſ. N° 448. p. 266. *Tranſlated from the* Latin.

THE ſtone diggers frequently found dead birds in a certain deep pit of this ſtone quarry, 800 paces from the *Pyrmont* ſprings ; and ſometimes different ſpecies of birds that were newly dead.

The

The Doctor, curious to find out the cauſe of the birds death, at firſt ſuſpected ſome mineral poiſonous matter, like cobalt or orpiment, intermix'd with the ſtones, and diſſolv'd by the rain water, of which the birds when thirſty, probably, drank.

Upon his going down into the pit, and ſtooping to take out the birds, he was immediately ſtruck with ſuch a penetrating ſulphureous ſtench, that giddy and aſthmatic, he was obliged to fly for it.

The Doctor therefore judged, that that ſuffocating ſulphureous ſteam was of the ſame nature and origin with what is obſerved evening and morning in a clear ſky and calm dry weather on the *Pyrmont* ſprings, where birds no ſooner draw near than they are ſuffocated; and ducks, that ſwim in a larger ſpring, commonly uſed for bathing, can ſcarce hold it out a few moments, but die away immediately.

The Doctor indeed thought it would be far from adviſeable, but rather a dangerous and noxious experiment, to make more ſuch vents and ſpiracles for the *Pyrmont* ſprings, and draw out from the bowels of the mountains the ſulphureo-ſpirituous ſteams that ought rather to be ſuppreſs'd and pent up for impregnating and ſharpning the ſubterraneous waters.

For this reaſon, therefore, and by the Doctor's advice, the Prince of *Waldeck* diſcharged the workmen from digging deep into the quarry, to avoid giving vent to the mineral vapours.

However, for curioſity and experiment ſake, the Doctor obtain'd of the Prince a ſpot of ground for a dry bath of ſix foot ſquare, which he cauſed to be arch'd over with ſtone, and cloſed up with a door.

This ſmall cell, order'd in this manner, can ſo much the leſs draw off the ſulphureo-ſpirituous virtue of the waters, as it is certain that that vapour does not always perſpire, but only when the weather is fair and calm, and the ſky ſerene, with eaſterly and dry, northerly winds, not ſtormy, but gently blowing, and with fogs and vapours ariſing before thunder and lightning.

The ſteam likewiſe is only found morning and evening; and as the ſun gradually aſcends, ſo the vapour deſcends, and towards noon retires under the ſtones, and entirely vaniſhes; about evening, it by degrees returns, and after ſun-ſet it comes out again very plentifully.

This vapour is not unlike fogs and watry exhalations; and yet it is never viſible but in the rays of the ſun, when the

tremulous motion of the vapour appearing like lightning, and arising at very short intervals from the stones, presents itself to view.

Generally the exhalation keeps in an horizontal direction, and scarce ascends and exerts its force beyond 1, 1 and ½ and two foot: So that if one go down into the pit, and stand upright therein any time, and not bend the head below the said horizontal line, he shall not perceive the smell.

Yet at certain times, especially when the sky is calm, serene and very dry, and pregnant with thunder and lightning, the vapour ascends and exerts its suffocating force, 5, 6 foot and upwards: But this seldom happens.

The chief phenomena and experiments of this mineral exhalation, hitherto observ'd, are the following.

1. Upon going into the cavern and keeping the body and head erect, one shall perceive no smell at all; and upon standing a few minutes in it the feet grow warm, and the steam immediately penetrates thro' the thickest shoes, and like burning nettles excites in the skin a stimulating sensation, that gradually extends to the legs and thighs, and warms the lower parts of the body in such manner, that one would think himself amidst a fire.

Upon standing still for a short while in this manner; a very plentiful sweat is raised, first in the inferior parts, and at length over the whole body, without any trouble or anxiety.

2. Upon bending the head towards the bottom of the pit, a very strong smell is immediately perceiv'd, respiration suppressed, an acrimony felt in the eyes, which makes them water, like garlick or horse-radish; the mouth and jaws filled with a sulphureous taste, the head affected with a vertigo and drowsiness; so that to avoid being suffocated and falling down, one is obliged to retire.

3. All kinds of insects, as flies, butterflies, beetles, &c. as soon as they come near the vapour, immediately drop down and die.

4. Sometimes the smaller birds, if the steam be very copious are suffocated the moment they come into it; and oftentimes for a few moments, as seis'd with convulsions, they flutter about and leap, open their becks, breathe short, and imitate the gestures of animals included in an exhausted receiver; and at length they die.

If the time be exactly observ'd when the birds drop down and begin to faint, but are not quite dead, and they then be speedily

ſpeedily brought into the open air, but eſpecially if water be pour'd into their mouths; they gradually awake as from a deep ſleep, and revive, and ſoon return to themſelves in ſuch manner as to become perfectly well, and continue ſo ever after.

And thus the Dr. has ſeen a bird for ten times in one day fall into ſuch ſwoons and recover again, and continue well for a long time after.

5. The larger birds, and tame fowl, as hens, ducks, geeſe, &c. hold it out longer in the pit, eſpecially if they ſtand with their long necks above the horizontal line, or by repeated leaps get above the ſphere of the vapour's activity, and ſo at times breathe the free air: But if the vapour be copious or if their heads be kept down to the bottom of the pit, they at length undergo the ſame fate with the ſmaller birds.

6. Quadrupeds; as dogs, cats, ſheep, &c. the larger and ſtronger they are, the longer they continue without being ſuffocated, and at length they die as birds do.

Yet they recover much more eaſily in the free air, eſpecially if they be plunged into water, or a deal of it be pour'd upon them.

7. This ſteam immediately extinguiſhes fire, eſpecially flame, and candles, whether included, or not, in tranſparent veſſels.

It is an agreeable phenomenon if a torch made up of a bundle of ſtraw be kindled and inclined to the bottom of the cavern; for, it is immediately extinguiſh'd; but rais'd again above the ſphere of the exhalation into the free air, it directly breaks out again into flame; and thus alternately, as often as one pleaſes.

8. When the vapour is copious and ſtrong, gunpowder cannot be fired at the bottom of the pit.

Sparks, ſtruck by a ſteel from pyrites or flint, may fall upon gun-powder, but not fire it.

In 1724, when firſt the Dr. caus'd wall in and arch over his vaporous bath, he thought with himſelf how he might apply to the benefit of mankind ſo ſurpriſing a vapour, which is much more ſubtile and penetrating than any of our chemical ſpirits.

For, he did not find it arſenical nor corroſive, as that from burning ſulphur; not inhering to, nor corroding the lungs, but only retarding and taking away one's breath and reſpiration: Yet not ſo ſuddenly in man, as not to give him ſufficient time to retire.

For

For experiment ſake the Dr. has frequently ſtaid in the cavern as long as he could bear the vapour, till he was almoſt faint and ſuffocated; he has ſuck'd it in with his mouth wide open, and at length he has jumped into the open air; He never felt any bad effects from thence, but rather found his breaſt and reſpiration become lighter; being troubled with a catarrh and cough he ſometimes found that catarrhous obſtructions and collections of matter were by means of this fumigation diſſolved and diſcuſſed.

There is not under the ſun a ſhorter and eaſier method of procuring ſweat: For, after ſtaying a few moments in this vaporous pit, ſweat plentifully breaks out all over the body.

Some country people, upon going ſometimes into the cavern, affirm that ſwellings of the legs, rheumatiſms and arthritic pains have been very much reliev'd by this ſteam.

But becauſe the exhalation ſometimes exceeds its uſual ſphere, and is then too ſtrong and intolerable, the Dr. was afraid leſt ſome unwary raſh perſon ſhould ſtay too long in the cavern and endanger his life; he therefore choſe rather not to make any experiments than endanger people's lives.

Barometers and thermometers, put into this vaporous pit, ſuffer no change; but remain in the ſame ſtate as in the free air.

The Effects of Dampier's Powder, *in curing the Bite of a mad* Dog; *by Mr.* Fuller. Phil. Tranſ. N° 448. p. 272.

MR. *Fuller* had us'd the *lychen cinereus terreſtris* with black pepper upon dogs, and always with ſucceſs; and ſome years before, a mad dog or cat (he forgets which) had bit ſome children and the mother at *Battle*; and mixing the *Lychen* according to *Dampier*'s direction, they all took it, as well as a dog or two that had been bit, and none of them had any bad effects from the bite.

Chriſtmaſs, 1737. a neighbour's ſervant going to ſearch whether a dog ſuſpected to be mad had been wormed (which dog died mad in 3 or 4 days after) was bit very much in both his hands: He went to a perſon who had had ſuch ſucceſs in uſing the *lychen cinereus terreſtris* with pepper for the bite of a mad dog, as to be applied to far and near, and who aſſured Mr. *Fuller* he would venture his life, that he cured any man or animal that was brought to him within 3 or 4 days after the bite. The ſervant took his medicine every day; about 10 or 11 o'clock he complain'd of a violent heat and pain in his head, which Mr. *Fuller* ſuſpected was the effect of the bite, and not the medicine

dicine: But after he had taken it for ſuch a ſtated number of days, he grew better, and continued well ever after. He had tied his fingers with ſhoemaker's *ends* (which are often uſed for a cut) and they were all very much inflamed and very ſore. Mr. *Fuller* made him take them off, and all his plaiſters, and waſh his hands with ſalt and water, and in a fortnight's time they were quite well.

Another Caſe of a Perſon bit by a mad Dog; *by Mr.* David Hartley *and Mr.* Francis Sandys. Phil. Tranſ. N° 448. p. 274.

ABOUT the latter end of *Nov.* 1732, Mr. *Soame*'s groom was bit in the hand by a mad dog, ſo as to fetch blood. On the 4th day, when Mr. *Sandys* firſt ſaw it, the wound was heal'd; but it was open'd again by him, and kept ſo for ſome time, but at laſt heal'd ſooner than was intended, thro' the ſervant's neglect.

He was bled, took a purge, after that half an ounce of *pulvis Antilyſſus* (vide *Phil. Tranſ.* N° 237 and 443) every morning for three mornings; and was order'd to go into cold water every day for ſome time; but he neglected it after the third day: Beſides, Mr. *Sandys* order'd him to forbear all meats, and drink nothing but water. He continued in this regimen for about five weeks; then finding himſelf well, would confine himſelf no longer to it.

On the 7th of *Jan.* following he was ſeized with a ſickneſs, vertigo, and faultering in his ſpeech and memory; and at laſt his vertigo increas'd to ſuch a degree that he fell down twice in the ſpace of half an hour; and the laſt time did not recover his ſenſes, till he was put to bed, and blooded to the quantity of 18 or 20 ounces. The patient continu'd all night reſtleſs and ſullen, and in the morning was blooded again to the quantity of 15 ounces. Mr. *Hartley* was ſent for and came about 8 at night, and found him very ſullen, thirſty, but averſe to drinking, and his pulſe quick and hard. He ordered him to be put into the cold bath: About midnight his pulſe riſing, the Dr. ordered him to be blooded to the quantity of 16 or 18 ounces: The patient continued reſtleſs all night. About 8 in the morning he went into the cold bath again. About 10 Mr. *Hartley* going away left it as his opinion, that the could bath and bleeding ſhould be freely repeated, as the circumſtances ſhould require. About noon Mr. *Sandys* came and bled the patient to the quantity of 18 or 20 ounces: He continued reſtleſs all this night. Upon Mr. *Sandy*'s aſking the patient, whether his

his averſion to drinking proceeded from any pain in ſwallowing, or ſome other cauſe? He made anſwer it was from a pain in ſwallowing.

Next morning the patient's ſtrength not being at all diminiſh'd, and his pulſe continuing full as vigorous as ever, Mr. *Sandys* bled him again to the quantity of 15 or 16 ounces; yet he ſtill continued the ſame, and took the ſame care of his horſes as uſual. Mr. *Sandys* going away left orders, that as long as the following ſymptoms, *viz.* reſtleſſneſs, ſtrength, and averſion to drinking continued, the patient ſhould be blooded freely, and put into the cold bath.

He was blooded twice more within the week; ſo that the whole quantity he loſt in that time was about 120 ounces.

After the laſt bleeding his ſymptoms diſappear'd, and he grew weak, low ſpirited and ſleepy: Then he went 8 times into the cold bath. He did not take any medicines during his whole ilneſs.

N. B. This perſon continued well ever ſince, Anno 1738.

New Experiments on Ice by the Abbé Nolet, Phil. Tranſ. N° 449. p. 307.

1. ICE, that begins to melt, and water, that begins to freeze, have always the ſame degree of cold.

2. That cold may be increaſed by a mixture of ſalts.

3. It has been thought for a long time that ſaltpetre was the fitteſt to heighten the cold of ice, but experiments have ſhewn, that few ſalts do it ſo little as that ſalt. Mix one part of fine ſaltpetre with two parts of pounded ice, and M. *Reaumur*'s thermometer will deſcend in it but 3 degrees and ½ below the freezing point:

What had caus'd this miſtake is, that people generally made uſe of ſalt-petre of the firſt or ſecond melting, as being the cheapeſt: But, that ſalt-petre not being purified contains a great deal of ſea-ſalt; and it was in proportion to the quantity of ſea-ſalt, that the effect was the greater.

From this laſt obſervation one may deduce an advantageous method for trying gun-powder. For, as of the three ingredients of which it is made up, ſaltpetre is the only one that can increaſe the cold of ice; if one part of gun-powder, or a little more, be mixt with two parts of ice, and it increaſes its cold more than three degrees and a half, it is a ſign that the ſaltpetre contain'd in it is not well purified: And the beſt gun-powder is that which does leaſt increaſe the cold of ice.

4. Sea-ſalt, that is, the bay-ſalt, which is commonly uſed at table

table in *France*; and that which is immediately taken from the mines, call'd *ſal gemmæ*, do, for the moſt part, give the greateſt degree of cold: For, pot-aſh ſometimes gives a little more, but generally leſs. Sea-ſalt mixt with ice in the aforeſaid proportion gives 15 degrees of cold on M. *Reaumur*'s thermometer, and *ſalgemmæ* 17.

5	Aſhes of green wood	3 degrees
6	ſea-coal	2
7	vitriol	2
8	tartar	10
9	common pot-aſh (in *France*, call'd *ſoude ordinaire.*)	3
10	Pot-aſh made of *vreck* or ſea-weed	11

This laſt pot-aſh may be ſubſtituted inſtead of ſea-ſalt, for making ice-creams, in places where ſalt is dear, as in *France*, where it is ſold for 10 ſols a pound.

1ſt. Becauſe in *France* this pot-aſh is ſold only for two ſols and a half a pound.

2dly. Becauſe, not freezing ſo faſt, it does not ſpoil the creams, by reducing them to iſicles.

3dly. Becauſe ice-creams, made this way, will keep longer in a condition fit to ſerve at table.

11.	Sugar gives	4 degrees
12.	Allom	1 $\frac{1}{2}$
13.	Salt of glaſs	10
14.	*Sal ammoniac*	12 $\frac{2}{3}$
15.	Quick-lime	1 $\frac{1}{4}$
16.	*Sal Glauberi*	2

17. The cold of ice may ſtill be conſiderably increaſed by a mixture of ſpirit of wine; about a drinking-glaſs full of it to a pound of beaten ice.

18. The cold of ice will not increaſe, unleſs the ice melt.

Experiments. Put into one veſſel four ounces of ice pounded very ſmall, and into another veſſel two ounces of ſea-ſalt; ſet the two veſſels in a mixture of ice and ſalt, which is ſtill to be renew'd, till, by means of the thermometer, you find, that the ſalt and the ice of the two firſt veſſels have acquir'd each of them ten or twelve degrees of cold; then mix your ſalt with your ice, and this mixture will not increaſe the degree of cold the ingredients had acquir'd, becauſe the mixture does not melt.

But if, instead of salt, you mix with your ice spirit of nitre, cool'd to the same degree as the ice; as this last is liquid, it will melt the ice, and considerably increase its degree of cold.

19. Salt, mixt with water, increases its degree of cold.

20. Of all salts, *sal ammoniac* gives the greatest degree of cold: So that if that salt has been cool'd in ice; and then one part of it be thrown into two parts of water, cool'd to the same degree in ice, that water will become colder than ice, and will freeze other water thrown into it in a small quantity.

This last observation may be applied to the cooling of liquors, where no ice is to be had: For, there is hardly any place, but what has wells; now the water of a well moderately deep wants about eight or ten degrees of the cold of ice; and *sal ammoniac*, being cool'd beforehand in the well, will, by mixing with some of the water of that well, come very near to the cold of ice.

An Observation of the Magnetic Needle, *being so affected by great Cold, as not to traverse, by Capt.* Christopher Middleton. Phil. Trans. N° 449. p. 310.

IN *Phil. Trans.* N° 418. Captain *Middleton* made mention of a strange phenomenon, relating to the sea-compass, which he had frequently observ'd, when among the ice in *Hudson's Bay*; to wit, that the magnetic virtue of the needle was so far lost or destroy'd, that it would not traverse as usual, even when the ship was in a considerable motion: And in his voyage thither in 1737, he observ'd that the compass would not move at all, any longer than the quarter-master kept touching it.

There was then much snow on the land, and several isles of ice round them, and the sea not very smooth. The Captain ordered one of the compasses to be brought into the cabin; but he did not find it any better, till it had stood near the fire about a quarter of an hour; and then it began to traverse very well. He then order'd it to be placed in the binnacle, and another to be brought into the cabin; changing them alternately thus every half hour; and he found by this means he could make them traverse, as well as in any other part of the world. He was obliged to continue this practice, 'till they got near 100 leagues from the coast, but afterwards he had no occasion for that trouble.

What should be the cause of this surprising phenomenon, he cannot conjecture; being certain that the compasses, as to their mechanical structure, were very perfect, and answer'd very well both before and after, during the whole voyage. There is never any

any oil uſed to make them move eaſily: For, in that caſe it might often congeal, and ſtop the motion of the chard. But whether the cold of the climate hath a power to deprive the needle of its virtue for a time; or that the friction is thereby increaſed to ſuch a degree, as that it cannot be overcome by the magnetiſm, he could not ſay; but the fact is certain and ſurpriſing.

An uncommon Palſey *of the* Eye-lids; *by Dr.* Andrew Cantwel. Phil. Tranſ. N° 449. p. 311.

A Nun about 30 years of age was troubled with as ſingular a diſorder as the Dr. ever heard of; *viz.* an intermitting periodical palſey of the eye-lids, which began every evening about 6 o'clock, with a defluxion from the great *canthus* of a whitiſh matter of ſome conſiſtence: So that ſhe continued blind till next morning, and then recover'd the uſe of her eye-lids, as before.

This diſorder held her for four months: from which time all remedies order'd her by her phyſician proving ineffectual, ſhe was ſent to *Balleruc* for the benefit of the waters.

The Dr. had a fair opportunity (as the patient and he lodged at the ſame houſe) of obſerving the effects the waters had on her. She was pump'd on the back part of her head and neck ſeven times, without receiving any ſenſible benefit: The ninth time her diſorder ſeiſed her an hour later than uſual, and the defluxion became leſs and thinner. The next evening it became two hours later, and the following night ſhe had as much command of her eye-lids as ever. She took the *douche* (for, ſo they call this way of pumping) the next morning and evening, and was entirely cured.

The Dr. ſat with her an hour that evening, carefully obſerv'd her eye-lids by candle light, and aſk'd her ſeveral queſtions on her diſorder. She open'd her eyes as well as the Dr. did his, and ſet out next day for *Montpelier*.

An Account of a Man *whoſe Arm with the Shoulder-blade was torn off by a* Mill, *on the* 15. *of* Aug. 1737; *by Mr.* John Belchier. Phil. Tranſ. N° 449. p. 313.

ONE *Samuel Wood*, about 26 years of age, being at work in one of the mills near the *iſle of Dogs* over againſt *Deptford*, and going to fetch a ſack of corn from the further part of the mill, in order to convey it up into the hopper, careleſsly

leſsly took with him a rope, at the end of which was a ſlip-knot, which he had put round his wriſt; and paſſing by one of the large wheels, the cogs of it caught hold of the rope, and he not being able to diſengage his hand inſtantly, was drawn, towards the wheel, and rais'd off the ground, till his body being check'd by the beam which ſupports the axis of the wheel, his arm with the ſhoulder-blade was ſeparated from it.

At the time the accident happen'd, he ſays, he was not ſenſible of any pain, but only felt a tingling about the wound, and being a good deal ſurpriſed, did not know that his arm was torn off, till he ſaw it in the wheel: When he was a little recover'd, he came down a narrow ladder to the firſt floor of the mill, where his brother was, who ſeeing his condition ran down ſtairs immediately out of the mill to a houſe adjacent to the next mill, which is about 100 yards diſtant from the place where the accident happen'd, and alarm'd the inhabitants: But before they could get out of the houſe to his aſſiſtance, he had walked by himſelf to within about 10 yards of the houſe; where being quite ſpent by the great effuſion of blood, he fainted away and lay on the ground: They immediately took him up, and carried him into the houſe, and ſtrew'd a large quantity of loaf-ſugar powder'd into the wound, in order to ſtop the hemorrhage, till they could have the aſſiſtance of a ſurgeon, whom they ſent inſtantly for to *Limehouſe*; who brought with him an *apparatus* for a broken arm, which he underſtood by what he could learn from the meſſenger to be the caſe: However, he ſent home for proper dreſſings, and when he came to examine particularly into the wound, in order to ſecure the large blood-veſſels, there was not the leaſt appearance of any, nor any effuſion of blood: So having firſt brought the fleſhy parts of the wound as near together as he could, by means of a needle and ligature, he dreſs'd him up with a warm digeſtive, and applied a proper bandage. The next morning he open'd the wound again, in company with two ſurgeons more: and not perceiving any effuſion of blood at that time, he dreſs'd him as before, and ſent him in the afternoon to St. *Thomas's Hoſpital*, where he was admitted a patient under the care of Mr. *Ferne*, from which time he was conſtantly attended, in expectation of a hemorrhage from the ſubclavian artery: But there being no appearance of freſh bleeding, it was not thought proper to remove the dreſſing during the ſpace of four days; when Mr. *Ferne* open'd the wound; at which time likewiſe

there

there was not the leaſt appearance of any blood-veſſels: So he dreſs'd him up again, and in about two months time the cure was entirely completed.

Upon examining the arm within a day or two after it was ſeparated from the body, Mr. *Belchier* found the *ſcapula* fractur'd tranſverſely, as were likewiſe the *radius* and *ulna* in two places: But whether theſe bones were fractured before the arm was torn off, the man could not poſſibly judge.

The muſcles inſerted into the *ſcapula* were broke off near their inſertions, but the muſcles ariſing from the *ſcapula* came away with it entire.

The *latiſſimus dorſi* and *pectoralis* were likewiſe broke off near their inſertions into the *os humeri*.

The integuments of the *ſcapula* and upper part of the arm were left on the body, as alſo the clavicle.

But what is very ſurpriſing is, that the ſubclavian artery, which could never be got at to be ſecured by art, ſhould not bleed at all after the firſt dreſſing; the artery being ſeparated ſo happily, that when its coats were contracted, the fleſhy parts preſs'd againſt its mouth, and prevented any effuſion of blood.

A Bullet *lodged near the Gullet for almoſt a Year*; *by* George Lord Carpenter. Phil. Tranſ. N° 449. p. 316.

THE late Lord *Carpenter* was wounded, at the defence of the breach of *Bribuega* in *Spain*, in the mouth, by a ſmall muſket-ball, which, having taken away part of his upper lip, beat out all his teeth, excepting two, on one ſide; broke and ſplinter'd part of his upper jaw-bone, went thro' his tongue, and lodged itſelf near his gullet, where it remain'd fifty one weeks and three days, before it was extracted.

The ledge which was made upon the bullet by the two foreteeth, lying almoſt by the gullet, and continually grating upon it, occaſioned an intolerable pain, and hindering him from ſwallowing any thing but liquids, it brought him ſo low, that his life being deſpair'd of, to make a final trial, his tongue was drawn out as far as poſſible; and one of the ſurgeons feeling the ball with his probe, which he then took to be a piece of a tooth (ſeveral pieces of teeth having been beat into his tongue by the bullet) and endeavouring to extract it, he took hold of the ledge with his forceps, and pull'd the ball out, after which he recover'd in a few weeks.

The marks of the foreteeth were to be ſeen on the bullet, and where it flatted upon the jaw-bone.

There

There is an extraordinary account of a gun-ſhot wound in *Phil. Tranſ.* N° 320, being the caſe of one Dr. *Fielding* who was ſhot in near the eye; and after 29 years the bullet was cut out near the *pomum Adami*.

An obſtruction of the Biliary Ducts, *and an Impoſthumation of the Gall-bladder, diſcharging upwards of* 18 *Quarts of* Bilious Matter *in* 25 *Days, without any apparent defect in the Animal Functions; by Mr.* Claudius Amyand. Phil. Tranſ. N° 449. p. 317.

ONE Mr. *Le Grange*, about 50 years of age, of a ſallow bilious complexion, died of an abſceſs in the gall-bladder.

Dr. *Vatas* his phyſician in ordinary ſays, that about 14 years before, this Gentleman was afflicted with a tertian ague, which was cured by the bark; and from that time had complained of a ſenſe of weight, and ſome uneaſineſs and hardneſs in the region of the liver and *borborygmi*, which were reliev'd by frequent purgations; notwithſtanding which, he had enjoy'd all the appearances of health, till about four months before his death, when ſome ſymptoms of the jaundice firſt began to appear on him, which had greatly increaſed five or ſix weeks before he died, when he began to complain of ſhooting pains on the right *hypochondrium*; which was ſoon follow'd with a hard inflammatory tumour there, tending to ſuppuration.

May 4. Mr. *Amyand* met Dr. *Vatas* and Mr. *Fiquel* his ſurgeon, in order to open a large abſceſs pointing below the cartilages of the ſecond and third ſpurious ribs on the right ſide. It was determin'd to open it immediately with a lancet, whereupon a pint of a purulent fetid matter was diſcharged. The aperture being large, and the dreſſings eaſy, by the next day it was found, that a very large quantity of *ſanies*, and ſome *pus* left in the bag, had found a vent; and this was ſo great, that it was thought proper to renew the dreſſings twice a day.

This had the deſir'd effect ſo far, that from this time the matter decreaſed daily till the 12th of *May*, when during the night, the wound had diſcharged near two quarts of matter of a ſaffron colour, intermixt with large flakes and thick lumps of a coagulated lymph or jelly, tinged of a deep yellow; and what ſurpriſed no leſs was, that upon dreſſing, way was made for the diſcharge of about a quart more of the ſame, as the orifice of the burſted bag was enlarged, to favour the coming out of the large flakes and lumps of jelly, obturating at times this orifice:

During

During this day the discharge was very great, and at night about a pint more of the same matter was emptied: From this time a short and thick *cannula* was left in the opening of the burſted bag, this causing a more easy and constant discharge; and a vulnerary injection, strongly saturated with spirit of wine, had the good effect to diminish it very considerably; but yet it continued so very great, that there was just reason to apprehend the patient would soon sink under so great a flux of this bilious matter, and the rather as his stomach and rest failed him; but the discharge daily lessening, and his appetite and rest returning in proportion, he recovered strength enough to be able to walk: All this while the appearances of the jaundice were wearing off, the urine was returned to its natural colour, and the patient had regularly a natural stool every day, till about 8 days before his death; when his body becoming costive, the physician found it necessary to discharge the *fæces* by clysters and lenient purges: Whilst Mr. *Amyand* attended him, his belly was always free from fullness or tension, being soft and lank, and he less troubled with wind, than he had been for many years before: Two days before he died, he went to air himself in another room and caught cold: This is presumed to have occasioned a fever, followed with a lethargy, in which he continued till the 29th of *May*, when he died.

Upon dissection, it was observed that the patient was not near so extenuated, as might have been expected, after so great a discharge of bile and lymph for 25 days; for, much fat was still observed under the skin and elsewhere, and his flesh not much sunk from the natural state, but the blood-vessels were found extremely empty: In the *abdomen* the caul or *omentum* was shrivel'd up, and adher'd to a great bag or *cystis*, affixed to the inside of the great lobe of the liver, and stretching from thence along the right flank, over one half of the kidney on that side: The left lobe of the liver was removed from the left side to the right, not reaching farther than the right edge of the *Cartilago ensiformis*, and the *pylorus*: The *ligamentum latum suspensorium hepatis* was drawn backwards into the right *hypochondrium*: The liver was of a natural colour, but very small, and more decayed and wasted in proportion than the other *viscera*, but as free as they from any preternatural adhesion, obstruction or induration; and the bag or *cystis*, arising from it, strongly adhering by its outside only, to the *peritonæum*, down to the right kidney.

Upon

Upon paſſing the finger thro' the wound in the integuments, it entered firſt into a cavity made between the *peritonæum* and the outſide of the *cyſtis*, in which the matter of the *abſceſs* had been lodged, and then thro' a hole in the *cyſtis*, or grand bag thro' which the great collection of bile in this *ſaccus* had afterwards made its way; and it was obſerved that the ſtrong coalition of this bag to the *peritonæum*, round that part where the *pus* had been collected, had ſhut up all communication with the cavity of the belly, and thereby prevented any extravaſation into the *abdomen*.

Now the bag or *cyſtis* being ſeparated from the *peritonæum*, and this and the liver ſpread on a board, it was obſerved that the matter had been collected in the gall-bladder, without affecting the liver itſelf: The *veſica fellis* was become a very large bladder, and extended ſo as then to appear capable of containing three pints, or more; it was nearly as broad as long; it aroſe very broad from the inner ſurface of the right lobe of the liver, which it occupied about 10 inches in circumference or more; its bulk had removed the ſtomach and *pylorus* from their natural ſituation, and preſſed them far under the left hypochonder, and that part of the *colon* placed naturally on the right kidney, forwards upon the ſpine; its ſurfaces were rugged and unequal, as that of a *potato*, and its coats thick and horny, forming ſeveral tumours, elongations or expanſions, of different ſizes and figures; one of which, as large as a hen's egg, was full of a cretaceous matter, intermixt with hard white ſtones: This cretaceous bag was made in the duplicature of the gall-bladder, but had no communication with, nor opening into it, which, ſeveral other tumours appearing of the ſame kind, had: Whence it was preſumed that ſome very ſmall pieces of cretaceous matter, found in the great bag, might have dropt from them into it, but it is more likely we had dropt them there, becauſe nothing like them had been diſcharged thorough the wound. The outward opening in this bag anſwer'd in the cavity of the *abſceſs*, wherein inciſion had been made, as this latter was formed between it and the *peritonæum*. In the bag were found about two ounces of the ſame bilious matter, which had all along been diſcharged; which being computed muſt be equal to, if not exceed the quantity of 18 or 20 quarts, during the 25 days the patient liv'd, from and after the opening of the tumour.

It has been obſerved, that the liver was in a natural ſtate, and that the matter collected in the gall-bladder had not in the leaſt

leaſt wounded or affected the liver itſelf: So that the large quantity of bile and lymph daily diſcharged thro' the inciſion, muſt have proceeded from the internal ſurface of the diſtended gall-bladder.

This put them upon enquiring for the *radices cyſticæ* and the hepati-cyſtic ducts, namely, thoſe very ducts which *Giovanni Caldeſi* has ſo carefully traced in ſeveral animals, and delineated in his *Obſervationi Anatomiche al illuſtriſſimo S. Franciſco Redi*, 1687. and which *Verheyen* has diſcover'd in the bullock kind, but could not trace in human ſubjects; namely theſe ducts whereby ſo great a quantity of gall has been depoſited in the gall-bladder; for as much as the *ductus cyſticus* was obturated, whereby ſome anatomiſts have ſuppoſed the gall to flow back from the hepatic ducts: And upon diſſection, was traced a trunk like that, which *Bidlow* and *Winſlow* obſerv'd in man, and like that formed by ſeveral branches in the liver, and diſcharging itſelf into the gall-bladder.

The *ductus communis choledochus* was found empty, and opening, as uſual, into the cavity of the *duodenum*; but the *cyſtic* duct was compreſſed by the bag in ſuch manner, that nothing could paſs thro' it.

The *ſpleen*, *pancreas*, and all the other *viſcera*, were in a natural ſtate, ſaving (as hath been already obſerv'd) that ſome of them had changed their natural ſituations.

Upon the whole it appears, 1. That the animal functions have been noways vitiated by ſome of the *viſcera* having been diſplaced; and notwithſtanding that for 25 days the diſcharge of the bile thro' the wound had been ſo great, that little was left to paſs into the *duodenum*, nevertheleſs he digeſted his food well. The ſtools continued regular, till within a few days before he died; and even to the laſt the *fæces* all along retain'd their natural colour.

2. It may be obſerv'd, that the jaundice in this patient was not occaſioned by the obſtruction in the *cyſtic* duct, tho' that is imagined as a common cauſe of this malady: For, this obſtruction muſt have been of many years ſtanding, and this patient's jaundice was of a very late date. Nor was his jaundice owing to any retention of the bile in the *porus biliarius*, from the tumour continually preſſing that duct, and thereby obſtructing the free diſcharge of the bile from the glands of the liver into the *duodenum* and gall-bladder; nor even to the ſtrong compreſſion and total obſtruction of ſome, yea almoſt all the biliary ducts, *viz.* the *pori biliarii*, the *ductus hepaticus*, the *hepati-cyſtici*,

cystici, and the *ductus cysticus* and *communis choledochus*; the principal of which are seated in the cavity of the great lobe of the liver, under the pressure of this great and hard tumour, and under its increase for near 14 years together, obstructions and compressions generally accounted as primary and idiopathic causes of the jaundice; because no distemper like the jaundice had appear'd in this patient till a few months before his death; and no true jaundice till within a few weeks, and only then as the *abscess* formed in the neighbourhood of the liver had brought an inflammation there: But as all the symptoms of his jaundice began to wear off, soon after the *pus* had got a vent, *viz.* as the inflammation of the liver, brought on occasionally by a suppuration in the neighbouring parts, wore off, and some days before the bursting of the gall-bladder, it does not appear unlikely, this inflammation of the liver was the pathognomonic cause of the jaundice here; which inflammation of the liver, as it was accidental; so the jaundice occasion'd thereby was actually removed soon after a vent was made for the purulent matter, which had occasion'd this inflammation.

N. B. The figures refered to were done by memory.

A (Fig. 2. Plate XII.) represents the external surface of the right lobe of the liver.

B. B. Parts of the same.

C. C. The ligament which suspends the liver to the diaphragm.

D. D. The ligament which suspends it to the *cartilago ensiformis*.

E. Part of the gall-bladder below the liver in its fore-part emptied of its contents, arising from the cavity of the right lobe, 10 to 12 inches in circumference.

F. F. F. F. Its adhesions to the *peritonæum*.

G. An opening into the external bag or *abscess*, or incision into it.

H. H. H. H. H. Elongations and inequalities in it.

O. O. O. O. The angles of the *cystis* open'd, shewing in its back parts an elongation opening into it at P.

A. A. A. (Fig. 3.) represents the concave side of the liver.

B. The umbilical vein.

C. The *vesica biliaria* emptied; which when full cover'd almost all the inside of the right lobe on its back part.

D. D. D. D. D. D. Several elongations or expansions of the *vesica* opening into the gall-bladder.

E. The cretaceous bag in its duplicature full of a chalky matter, intermixt with hard white irregular ſtones,

Some Obſervations on the above-mention'd Caſe; by Dr. Alexander Stuart. Phil. Tranſ. N° 449. p. 325.

THE Dr. points out what appears to him to be the mechanical and neceſſary connexion between the apparent cauſes and the effects in this uncommon caſe.

1. As to the original or prime cauſe of all the ſymptoms, to wit, the diſtention of the gall-bladder, now become a morbid *cyſtis* of an enormous ſize.

If we conſider the ſize and figure of the liver, the ſituation of the gall-bladder, with the courſe or direction of the biliary veſſels, from various places of the liver, towards that narrow ſpace where the *pori biliarii* open into the *cyſtis*, it will appear, that in almoſt every poſition of the body, at leaſt in an erect, ſupine and lateral poſition, ſome of theſe biliary ducts, terminating in the gall-bladder, are perpendicular or nearly perpendicular to the horizon and to the *cyſtis*: Therefore, as far as gravity takes place in the animal oeconomy, the bile deſcending by theſe ducts, will preſs upon the contents of the full *cyſtis* and its ſides, as a cylinder of that fluid, of the length of the ſecretory ducts or tubes, and of the diameter of the *cyſtis*.

Beſides this, the extremity of every one of theſe ſmall ducts conveys its fluid into the full *cyſtis*, as a wedge acted upon by the repeated ſtrokes, impulſes or preſſure of the circulating blood of the *vena porta*, where it ſupplies the gland at the origin of each ſecretory duct.

Therefore, by the known laws of hydroſtatics and mechanics, it is plain, that the force of this ſecretion of the bile into the gall-bladder is very great, and the quantity thereof conſiderable; ſufficient at leaſt to diſtend the *cyſtis* to an enormous pitch, where the diſcharge by the *ductus cyſticus* is not equal to the ſecretion by the *pori biliarii*, and the *ductus hepatico-cyſticus*.

Theſe ſaid powers do ſufficiently account for the diſtention of the *abdomen* in an *aſcites*, of the womb in geſtation, of the bladder in a morbid or voluntary retention of urine; as alſo of morbid impoſtems or tumours, and of the gall-bladder in the caſe before us.

But this diſtention could never have happened, without a total or partial obſtruction of the excretory duct, namely the *ductus cyſticus*,

Had this obſtruction been at once total, as when a *calculus* is thrown ſuddenly out of the *cyſtis* into the duct, and ſtops it entirely, he muſt have had the jaundice immediately, or very ſoon after: For, notwithſtanding the ſtrong powers above-mentioned, it would have been impoſſible for the ſides of the *cyſtis* to have yielded to ſuch a ſudden dilatation, no more than the womb in the firſt week of geſtation can be dilated to the pitch it is brought to in the ninth month, without a rupture: So that the dilatation here muſt have been very ſlow and gradual; and therefore the obſtruction muſt have been at firſt, and probably for ſeveral years, only partial; and the gall-bladder thus ſlowly diſtended, gradually yielded and gave way only for the reception of the exceſs of the ſecretion beyond the excretion; and ſo prevented the jaundice, or regurgitation of the bile into the blood.

This partial obſtruction of the *ductus cyſticus* may probably have been occaſioned by one of thoſe ſmall ſoft incyſted tumours, lodged between the membranes of the *cyſtis fellea*, near the origin of its excretory duct, containing a ſoft white pultaceous matter, with *calculus*'s or chalky concretions in its centre.

If this was the caſe, it is conceivable that while the contents of this ſmall incyſted tumour was fluid or ſoft, it might not be capable to obſtruct entirely the current of the bile thro' the excretory duct: But as the matter of it grew thicker, and its bulk increaſed, by preſſing gradually more and more upon the duct, the obſtruction muſt increaſe; and the formation of *calculus*'s, by their preſſure, muſt at laſt make the obſtruction total,

But as the cyſtic duct was at opening of the body, entirely coaleſced and obliterated, its vicinity and ſituation, with reſpect to theſe ſmall incyſted pultaceous and cretaceous tumours, cannot be preciſely determined; and therefore this is offer'd only as a probable conjecture.

The bulk, contents and adheſions of the gall-bladder to the right ſide, were without doubt to the patient a very ſenſible, and to others a viſible cauſe of his firſt ſymptom, *viz.* the increaſing weight he felt in the region of the liver, for 14 years before his death.

The current of moving humours in the animal body is always determined moſt ſtrongly to the place of leaſt reſiſtance: Therefore, by the partial obſtruction of the cyſtic duct, a greater quantity of bile than uſual will be forced upon the biliary ducts, leading directly from the liver into the great hepatic duct, to diſcharge itſelf by the *choledochus communis* into the *duodenum*, ſufficient for the moderate uſes of the animal oeconomy; tho'

not

not so perfectly sufficient, but that the peristaltic motion in the present case felt the want, or at least the defect of the cystic bile; so far as to become weak and imperfect; too weak to propel the *fæces*, or keep the elastic air within due bounds; and therefore the patient must be subject to flatulent distentions, and some degree of costiveness, only to be reliev'd by supplying the want of a sufficient natural *stimulus* of the gall, by the artificial *stimulus* of purgatives and clysters, to assist from time to time the expulsion both of the *fæces* and also of the *flatus*'s, for the ease of the patient, as was practised in this case.

As to the jaundice, it began to shew itself four months before the patient's death, and continued increasing till the external purulent tumour in his side was opened, and then it began to decline, and quite disappear soon after the gall-bladder burst.

It is easy to conceive, that so long as the gall, descending from the *pori biliarii*, could make its way into the *cystis fellea*, and dilate it; there could be no regurgitation of the bile into the blood; and therefore no jaundice.

But so soon as the purulent impostem began to form itself in the neighbourhood and contact of the distended gall-bladder, it encroach'd or press'd upon the *cystis fellea*, by the force of a great many vessels, pouring *pus* into the cavity of the impostem, urged on by the circulation of the blood, which is more forcible in these vessels than in those of the liver.

And therefore this purulent tumour increasing, will very forcibly incroach on the *cystis fellea* in contact with it; and not only hinder its farther distention, but even force the gall it contains to regurgitate or return again by the *pori biliarii* upwards, and from thence by the capillaries of the *cava*, into the blood; and so produce the jaundice, without raising an inflammation or obstruction in the liver itself, whose vessels and passages remain open, tho' the bile take a retrograde course in its biliary secretory ducts.

But so soon as this accessary pressure is taken off from the *cystis fellea*, by opening and emptying the purulent tumour or impostem in its neighbourhood, adjoining and adhering to it, the bile begins again to flow freely into the *cystis fellea*, and dilate it as before: Therefore, the regurgitation of the bile into the blood ceaseth, and the jaundice begins to decline.

Then so soon as the rupture or bursting of the gall-bladder happened, and it began to be emptied; all degrees of resistance being now entirely taken off from the *pori biliarii*, they throw out their contents so plentifully, that the hepatic ducts are gradually

dually fruſtrated by ſuch a ſtrong revulſion; the bile begins to flow all to the wounded and almoſt emptied *cyſtis biliaria*, and either very little or none at all to be carried by the *ductus hepaticus* to the *choledochus communis*, whoſe diameter and paſſage into the *duodenum* was found larger than uſual, but empty.

In this ſtate, which was the laſt ſtage of the diſtemper, the periſtaltic motion begins to fail, the expulſion of the *fæces* to be very tardy, or not at all to anſwer without the aſſiſtance of purging medicines or clyſters, which alſo had but a very ſlender effect; the patient ceaſeth to be nouriſh'd (tho' he took a competent quantity of food) and dies in a week after this coſtiveneſs began.

The degree of perfection of the natural, vital and animal functions in this perſon, during 14 years indiſpoſition, was certainly owing to the ſoundneſs of all the *viſcera*; and an almoſt ſufficient ſecretion and excretion of bile by the *ductus hepaticus* into the *choledochus communis*, whoſe cavity and paſſage into the *duodenum* was large and open; which could neither have been, nor have continued, without a continual and proportional flux of bile thro' it.

For, it is well known, that as ſoon as the fluids ceaſe to flow thro' their natural ducts, their ſides ſoon collapſe, coaleſce, and at laſt entirely ſhut up: Thus the *urachus*, and *canalis arterioſus Botalli* in the *fœtus*, ſhut up entirely ſoon after the birth: And the Dr. and Mr. *Amyand* had lately ſeen one of the *ureters* entirely collapſed and ſhut up, for want of a fluid from the kidney, which had ſecreted no urine for ſome time; having become a *cyſtis*, fill'd with a thick white pultaceous matter, nearly of a chalky conſiſtence.

Therefore, as the *ductus cyſticus* was found obliterated, and the *choledochus communis* large and open, it is plain, that no bile had for ſome time flow'd thorough the former, and that there was a conſtant ſupply from the *ductus hepaticus* to the latter, for the uſes of the animal oeconomy, till the wound or rupture of the gall-bladder, gradually abating its current by that channel, at laſt ſtopp'd it quite, and put an end to the patient's life in a few days after.

The moſt eſſential points in this caſe, bearing a conformity with what the Dr. obſerv'd in a former *Tranſaction*, concerning the uſe of the bile in the animal oeconomy, may ſo far ſerve to confirm it.

An Hiſtorico-phyſical Obſervation on the Neuſohl *Vitriolic Waters, commonly call'd* Cement-Waſſer; *by M.* Matthias Belius. Phil. Tranſ. N° 450. p. 351. *Tranſlated from the* Latin.

THESE ſprings are a mile to the north of the town of *Neuſohl*, in the large copper-mine, call'd *Herren-grund*: The time when they were firſt diſcover'd and obſerved, is uncertain: That they were not known in the days of *George Agricola*, the *German Pliny*, may be concluded from his ſilence about them.

After the ſacking and burning of *Neuſohl* in 1605 by *Botskay*, the miners, to ſave their iron tools, as their hammers, *&c.* and other effects, hid them in the mines; where remaining for upwards of a month, they were found in thoſe moiſt places cover'd over with a cruſt of copper, which was the thicker, the damper the place was where they had lain. From thence they therefore concluded, that the waters trickling down along the ſides of the mines, muſt be endued with a quality of producing copper, which at length gave occaſion to their making wooden trunks, for collecting theſe waters, as at *Schmolnitz*, and arching them over.

Now this compendious method of producing copper approved itſelf ſo much, that there are upwards of 20 ſuch places; two of which M. *Belius* gives an account of, whereby one may eaſily judge of the reſt.

The principal one is about 75 fathom perpendicular depth; and you deſcend to it in a winding manner by ſteps for 151 fathom: The water trickles down in ſingle drops from the ſides of the mine, and is received at firſt into a ſmaller veſſel, and after that into one larger, and divided into trunks. Into the leſſer are put the ſmaller pieces of iron, as horſe-ſhoes and the like, which in three or four weeks time are turn'd into copper, of the ſame form as before, but emboſs'd a little: And the water in this, has a much greater virtue than that receiv'd into the larger one: For, in the latter the iron corrodes ſlowly, and in ſuch manner, that at firſt only a ſlimy pellicle, of a yellowiſh colour, floats upon the ſurface of the water, which pellicle at length gradually adheres to the iron, when not entirely conſumed, like a fat ſlime, which the miners call *der ſchmund*; and which every month they carefully take from off the iron, and put in a peculiar high place to drain; and this they repeat till the iron, or the greateſt part thereof, be entirely conſumed.

The

The ſecond vault is 15 fathom lower, which from its form and ſituation, is call'd the *long vault*: For, being two fathoms in breadth throughout, it is 25 fathoms in length: In this the vitriolic water trickles more copiouſly than in the reſt of the vaults: For, beſides that it drops down from the ſides and roof of the mine, it has two conſtant ſprings, which continually yield a ſtream of water of the thickneſs of a ſtraw.

Both ſprings riſe from the ſouth; and the firſt is three paces to the left from the entry of the vault, and the ſecond five paces more inwardly: And that the waters may not be waſted, they are conveyed thro' ſmall pipes, partly into wooden trunks and partly into ſquare veſſels, into which at length are put old and new iron.

M. *Belius* obſerved that the trunks for receiving the water and the iron, are ſo carefully diſpoſed on the ground, that none of the water can be loſt: For what is receiv'd into one trunk, flows into another, and from this into a third, and ſo on; yet the efficacy of the waters is much diminiſh'd: For, in the firſt, the iron is conſumed ſooner and more ſtrongly; but ſlower and more weakly in the ſecond and third.

In this vault, from a middle wall, there trickles down a peculiar and more limpid ſort of water, which is therefore receiv'd into a peculiar veſſel: And into this the copper, made in the other vaults, is at length put, if they want to have it purer: For, this limpid water has the quality of rendering impure copper more fine and bright: Beſides the above receptacles for the vitriolic waters, there are up and down in the windings of the mines, damp places, where iron is tinged of a copper colour, which ſhews that all their moiſture is vitriolic.

As to the water itſelf, it appears greeniſh in the veſſels or reſervoirs; tho', in a clear glaſs, it be limpid, and tranſparent like cryſtal; it is inodorous, but of a vitriolic aſtringent taſte, and of a cold nature. M. *Belius* unwarily taſting a few drops of the water at the ſprings, had his lips ſhrivel'd up in ſuch manner, as thoſe, who are juſt recover'd from a fever. After having crept thro' theſe ſubterraneous paſſages for three or four *German* miles, he felt no pain in his lips, but a gentle kind of itching; but upon coming into the open air, they began at firſt to ſwell, and afterwards to ſuppurate: In other reſpects the water has the ſame efficacy, only where it happens to trickle down more copiouſly: For then it is more dilute, and ſlower in conſuming iron.

The

The water is so far from spoiling the wooden trunks and reservoirs, that receive it, that it hardens and makes them last longer than usual.

The vaults, in which the vessels are included, have no strong smell, nor any vitriol, which shoots every where else in this mine; and this M. *Belius* takes to be owing to the moister air, which dissolves it, and does not suffer it to coalesce into filaments or crystals: Yet in some vaults, where these waters trickle down, one shall find a whitish stone in one place, and quite bluish, like vitriol, in another: And likewise at the sides of the drifts, next the bottom of the vaults, there is a concretion of a kind of mean salt, which, mixt with a moist and yellow earth, is insipid, and friable like *lapis specularis*.

The miners, in desperate cases, do with great hopes of success, drink this vitriolic water as a medicinal potion, which at length causes either a purging or a vomiting, or both at the same time.

The use of it is safer in *ophthalmia*'s, if it be cautiously applied in the manner of a *collyrium*: 'For, vitriolic waters do 'service to the eyes', as *Agricola* says *lib.* II. *p. m.* 117. *de natura eorum quæ effluunt ex terra.*

The copper, which these vitriolic waters generate, is much finer, more ductile, and apter to smelt and fuse than the other kinds of copper; whereby goldsmiths have been induced to make bowls, drinking cups, and tobacco boxes of it.

While this copper remains in the waters, it is much more friable than when taken out of them: For then it hardens in some measure, by a closer concretion of its constituent particles.

The abovementioned slime is no other than indigested copper, precipitated from the water, and sticking to the iron; which is yearly carried to the copper-forge at *Neusohl*, and there refined into the purest copper, and that without any waste; by reason that the iron which is consumed by the vitriolic water leaves behind it a little heterogenous matter in the said slime.

Before these vitriolic veins of water were spoil'd by an inundation that happened within M. *Belius*'s memory, it is certain, a greater quantity of copper was produced from iron: For, in 1707 eighty eight centners of iron were turned into copper; whereas now scarce 20 centners a year.

Whence we may easily conclude, that that inundation greatly impair'd the pristine efficacy of the vitriolic waters; the veins, becoming more copious, entirely weakened the vitriolic spirit, which

which they carried along with them; and the few vitriolic vaults, there formerly were, produced more copper, than 20 vaults now; becauſe moſt of them do not produce ſolid copper, but ſlime only, which, as has been ſaid, muſt at length be refined in a ſtrong fire.

Thus far as to the diſpoſition of theſe vitriolic waters: M. *Belius* comes now to give an account of the experiments he himſelf and his friends made to find out their more intimate nature.

Ex. 1. A pound of the ſtrongeſt vitriolic water, gradually and ſlowly evaporated, was at firſt turbid, depoſiting a little dirty powder, which, afterwards boil'd to dryneſs, left behind it two ſcruples and a half of a greeniſh remainder; this remainder diſſolv'd in water, in the uſual manner, yielded a green ſolution; which at length filtred and evaporated, gave two ſcruples of cryſtals of vitriol. What remain'd of the powder was of a dirty colour, and weigh'd ſix grains: So that a pound (apothecary weight) of the vitriolic water ſcarce holds above two ſcruples of vitriol of copper.

Ex. 2. A pound of the ſame vitriolic water, precipitated with oil of tartar, became turbid and of a ſea-green colour; this, at length filtred, left a remainder in the ſtrainer; which dried, yielded two ſcruples and a half, with a little mean ſalt.

Ex. 3. In fine, a pound of vitriolic water in a glaſs accurately ſtopped, began to tinge inſenſibly of a copper colour, an iron wedge that had been put into it, with bubbles adhering to it here and there; the following day, the water became turbid and whitiſh, with whitiſh ſtreaks obſervable at the ſame time at the bottom of the glaſs, and about the wedge: And ſome days after, a dirty or copperiſh ſediment was obſerv'd on the ſame wedge.

From theſe experiments we learn.

1. That this water is really vitriolic, and plentifully ſaturated with vitriol of copper, and flows from metallic veins, in which the vitriolic pyrites is diſſolv'd.

2. That the ſame water corrodes and diſſolves iron, and precipitates its copperiſh particles; which inſenſibly put on the form of the iron, put into it, and to which they adhere; and this a more accurate inſpection of this copper abundantly ſhews: For, it is not formed into a ſolid and ſmooth maſs, but conſiſts of a vaſt number of little grains, which coaleſce into one friable body, like the ſpawn of fiſh.

And indeed it is a thing well known both to chemiſts and metallurgiſts, and even to ſuch as are but moderately acquainted with

with theſe arts, that one metal is precipitated by another: For, quickſilver, diſſolv'd in *aqua fortis*, precipitates ſilver; ſilver lead; lead copper; and in fine, copper iron.

Hence, upon diſſolving a bit of copper in *aqua fortis*, and afterwards putting into it a bit of iron, the ſame tranſmutation may be obſerv'd therein, as in the vitriolic waters; namely that *menſtruum* will corrode and diſſolve the iron, and at the ſame time the copper, mixt with the *menſtruum*, will be ſeparated from it, and ſubſide inſenſibly and ſlowly into the place of the iron.

Since theſe things are really ſo; we may hence refute the concluſions, raſhly drawn by ſome from the effects of this vitriolic water.

And 1. Since an equal bulk of copper is uſually got from this water, with the iron put into it; ſuch argue wrong who think, that the iron, corroded by the water, depoſites the copper particles it contain'd, as looſened from their tye, and entirely conſumes the remaining material parts, or cauſe them vaniſh.

2. Nor can the eſſential tranſmutation of iron into copper take place in this whole operation of nature; which alchemiſts and other pretended artiſts perſuade themſelves, and others; as if a more imperfect and ignoble metal could be entirely changed into one more perfect and noble: For, it ſufficiently appears from the above-mentioned experiments, and from phyſics, that the aforeſaid vitriolic water does not transform iron into copper, but only depoſite its copper particles, with which it was before impregnated. And M. *Belius* might from his own obſervation evince the quite contrary to what theſe pretended artiſts affirm: For, if iron and copper, metals nearly allied, cannot, by the help of nature itſelf, be transformed into one another, as, for inſtaace, iron become copper, much leſs can that be expected from art, even advanced to the greateſt pitch.

That among all the metals, copper and iron, are peculiarly allied to each other, M. *Henckel* has abundantly made out in *Pyriſt. pyritum.* p. 424. where at the ſame time, p. 422 he affirms that, amongſt ſo many experiments, he did not meet with any ſtone, or copper ore, which the magnet would not attract: So that the attractive virtue of the magnet next to iron, is obſerv'd to affect copper: And ſince iron and copper are metals that equally yield what is properly call'd vitriol; a thing which cannot be ſaid of the other metals under the ſame ſpecies and form:

form: For, they are of a like ſubſtance and colour, either green or blue; and were the pretended hermaphrodite vitriol found any where, it muſt neceſſarily be diſcover'd by infallible ſigns in the preſent operation of nature; and yet this never happens.

Moreover, that the vitriolic waters of *Neuſohl* derive all their virtue and efficacy, as mention'd above, from the vitriolic *pyrites* diſſolv'd in ſubterraneous *meatus*'s, is a thing both ſelf-evident, and confirmed by the works at *Schmolnitz*: For, the whole tract of that town, both on the ſurface and inſide, abounds in a vitriolic *pyrites*: So that the vitriolic water (*cement-waſſer*) ſprings up plentifully not only within the mines, but on their ſurface; whence it happens that it is at the ſame time ſtronger and more efficacious to precipitate copper than the water in *Herren grund*: And the people of *Schmolnitz* are wont, in dry weather, and when the vitriolic waters in the ſprings fail, to water abundantly, with common water from the neighbouring ſprings, the heaps of vitriolic *pyrites* landed from the mines, as alſo the old drifts; ſo that this common water, by elixating the vitriolic *pyrites*, does thereby acquire its virtue of corroding iron, and precipitating copper: Which happens thus; theſe common waters running together into wooden trunks and veſſels, do, by elixating the *pyrites*, acquire the ſame virtue and efficacy with the native and vitriolic waters.

A Bubonocele *or rupture in the* Groin, *and the Operation made upon it; by Mr.* Amyand. Phil. Tranſ. N° 450. p. 361.

ON the 8th of *October*, 1737, one Mrs. *Bennet*, 70 years of age, of a thin habit of body, had a return of a tumour in the groin, with unuſual pain, which was ſoon followed with a cruciating one in the belly, and ſuch colics, reachings, and excrementitious vomitings, as uſually attend the ſtrangulation of the gut in the *miſerere mei*. This came upon her unawares, and the diſtreſs ſhe was in made her forget that for 25 years before ſhe had had a ſwelling in the groin, as big as a haſel-nut, which ſeldom had given her any uneaſineſs, and which ſhe never ſuſpected to be a rupture. Latterly the patient had been more ſubject to colics than uſual, but that was imputed to bad digeſtions; and that day ſhe had uſed no motion capable of producing a rupture: So that it was by chance that Mr. *Deſpaignol*, who was ſent for the next day, diſcover'd the cauſe of the complaint. She was blooded, clyſter'd, fomented, poulticed and embrocated; but the complaints ſubſiſting

ing with a continual *ſingultus*, Mr. *Amyand* was call'd in the 11th.

The tumour was now oblong, about the bigneſs of a hen's egg, ſomewhat inflamed, yet not tenſe, nor ſo painful as to take much notice of it.

Upon the repeated uſe of the above-mention'd means, and of lenient purges and opiates, the vomitings and hiccough were at times ſtopped, and the patient made ſo much eaſier, as to ground hopes of ſucceſs; but as during ſix days the patient had had no paſſage, and the tumour could not be reduced, it was thought unſafe to delay the operation any longer. At this time ſhe was free from a fever, the belly was not tenſe, and ſhe had conſiderable intervals of reſt.

The tumour felt unequal, tho' it appear'd even, and pappy, as the tumours of the *omentum* generally are; and therefore of that kind which is always moſt difficult to reduce, the *omentum* not having that elaſtic ſpringineſs which favours the replacing of the guts.

Upon diſſection it was found embodied in the hernial bag, and that upon the external ſurface of the ſlits in the abdominal muſcles, the folds of it had formed a round protuberance, not unlike the *os tincæ* in the *vagina*, or like a *bourlet*, which, by compreſſing the gut, prevented its return into the belly, and by obturating the opening, as the gut preſs'd upon it, had ſtrangulated about an inch of the gut encompaſt by it in the *hernia*.

This being the 6th day from the beginning of this diſorder, the gut in that place was found of a very ſwarthy colour, but yet ſpringy; ſo that it was not quite mortified. It lay incloſed in a net, formed by the *omentum*, (like a fiſh in a fiſhing net) ſtrangulating the gut under its preſſure without the abdominal muſcles.

It was with ſome difficulty the *omentum* was torn off and ſeparated from the bag it was attach'd to; and as this lay in the way of the reduction of the gut, and almoſt ſphacelated; ſo it was cut off without any previous ligature, tho' its veſſels were turgid and large, as it was impoſſible to pull it out ſo as to make the ligature upon the ſound part of it; after which the reduction of the gut might eaſily have been made, without enlarging the annular ſlit: For, this made no ſtricture to prevent it.

But the quantity of the *omentum* within it being large and bulky, and the gut in a very crazy ſtate, it was thought more expedient to enlarge it, to make the reduction of the whole eaſy: Afterwards the *omentum* was detach'd from its adheſion round

round this place, and pull'd farther out; and a ligature being made upon the ſound part of it, that was alſo replaced in the belly, and the entrance ſtopt with a conical tent, dipt in the yolk of an egg and oil of St. *John's wort*; the belly was embrocated, and the dreſſings well ſecured: For, as the patient was very much oppreſſed with an aſthma; ſo ſhe was obliged to be ſitting in bed.

From this time the hiccough and excrementitious vomitings diſappear'd, but the reachings and vomitings continued near five days longer, before the *fæces*, detain'd above the ſtrangulated gut, could make their way downwards, tho' they were frequently invited by clyſters and lenient purges.

The patient was blooded immediately after the operation; and ſoon after took an emollient and carminative clyſter, which was repeated night and morning; and an oily laxative of two drachms of *manna*, and half an ounce of oil of ſweet almonds, in mint and ſmall cinnamon-water, every four hours.

At firſt the evacuations were exceeding fetid, black, griping and frequent; but they became more moderate, as ſhe took abſorbents and diluents; but yet ſo frequent, that it was thought proper to reſtrain them by gentle aſtringents; ſo that ſhe might be enabled to bear them.

In five or ſix days the ſtools had remov'd the tenſion, which appear'd on the belly after the operations; the reachings and vomitings, and the remaining ſymptoms went off; the wound digeſted well, and the patient continued in a mending way.

It has been already obſerv'd, that this old woman was very much afflicted with an aſthma: She had at times violent fits of it; and the 14th day from the operation ſhe had one, with a total ſtoppage of the diſcharge from her lungs, which choak'd her on the 17th day.

This caſe confirms Mr. *Amyand* in what he has frequently obſerved on the like occaſion; namely, that as the *omentum* is the principal obſtacle to the reduction of the guts in ruptures; ſo it is the occaſion of the greateſt accidents that attend that evil. It wraps up and encloſes the prolapſed gut, like a net, whoſe faſten'd end within the belly ſtrangulates the part detain'd in the rupture without the abdominal apertures, where it is confined; and is productive of ſuch folds in it, and preſſures of the gut wrapt up therein, as is oftener the cauſe of a ſtrangulation and *miſerere mei* than the tendinous ſlits of the external oblique muſcles in the inguinal rupture, or tendinous opening in the navel; which upon theſe is ſeldom found inflamed,

and

and can never contract ſo ſuddenly, as to obſtruct the return of the gut into the *abdomen*, when the *omentum* is wanting: Agreeable to which, it is rare to find any ſtrangulated rupture that is not attended by it.

The fatty ſubſtance of the *omentum* ſubjects it to inflammations, ſuppurations and putrefactions, that contaminate the neighbouring parts. It wants that elaſtic ſpringineſs the guts have, which favour the reduction in ruptures. It frequently ſtays behind when the guts are reduced, and therefore bars the patient not only from the benefit of retentive truſſes he ſtands in need of for his ſecurity, but it directs the gut into the rupture where it lies, the guts being very apt to ſlide down along it; and when it is fixt in the rupture, it too often pulls and draws into it the *cæcum* and *colon* it is attach'd to, and even the ſtomach itſelf, in proportion as the quantity of it in the rupture happens to be more or leſs; and therefore umbilical ruptures are the moſt dangerous of any: For, as the *omentum* lies over the guts, ſo it is always preſt in foremoſt, in the ruptures of this part, which, when large, will likewiſe cauſe an elongation of the bottom of the bladder that way, and a difficulty of urine, in proportion as the *urachus* attach'd there is ſtretch'd forwards towards the navel.

The pain, attending the *prolapſus*, ſoon ſwells the veſſels of the *omentum*, and that will fill up the apertures in the abdominal muſcles, thro' which the *viſcera* are fallen out, prevent their return, and bring on an inflammation: If by plentiful bleeding the veſſels emptied do not facilitate the return of the parts prolapſed, and all the conſequences that are generally obſerv'd upon the like occaſion; and if theſe do not operate ſoon, it is very ſeldom that any thing is got by the application and uſe of all the other means preſcribed.

It is, however, certain, that it is very dangerous to depend too long upon them; and that a ſuſpenſion of the ſymptoms is no ſecurity, whilſt the due courſe of the *fæces* is interrupted.

The caſe here mentioned may be a warning to others, not to delay too long an operation, whereby the parts are to be releaſed from confinement; and which oftener would be ſucceſsful, if it was not delay'd ſo long.

In the caſe of a rupture with a *miſerere mei*, ſome deny that excrements and clyſters from the lower guts can aſcend and be diſcharged thorough the mouth, upon a preſumption, that the ſtrangulation which prevents and ſtops the deſcent of a thin fluid downwards, muſt likewiſe prevent the aſcent, and eſpecially

ally of ſuch ſolid ſubſtances as are ſaid to be diſcharged upwards; and the rather that the *valvula coli*, and the *rugæ* or valves of the guts muſt impede the aſcent: But the fact is true; and there is no one converſant in practice but has obſerv'd *fæces* and oily clyſters diſcharged upwards.

If this be allow'd, it will follow, that in the rupture of the guts, there is a paſſage thorough their pipe; and conſequently, that the ſtrangulation muſt be leſs than it is generally aſcertain'd.

The inflammation of the guts inverts (but we do not know how) their periſtaltic action, and the diſcharge of the *fæces*; and that ſo long as that is continued: Inſomuch that this will continue even for ſome days after the reduction of the gut is made.

Parts inflamed and in contact will ſoon ſtick and coaleſce together. Pain is the indicating ſign of inflammation; and an inflammation is an intumefaction of the veſſels in the parts inflamed.

If then pain happen to be an attendant of a rupture, wherein the *omentum* is concerned; and the parts ſo inflamed continue in contact; that is, if the parts prolapſed in a rupture are not ſoon reduced, they will ſwell in the bag, and be knit together; and by filling up the opening, by which they had prolapſed, choak up the paſſage, clog and prevent the returning back, compreſs the guts under the preſſure, and ſtrangulate them more and more, in proportion as their bulk ſhall increaſe ſo long as the fluids can flow into the compreſt canals; in which they at laſt ſtagnate, and upon extravaſation ſuppurate; or the mortification of the parts compreſt enſues.

A Pin *taken out of a Child's Bladder*; *by Mr.* William Gregory. Phil. Tranſ. N° 450. p. 367.

MR. *Gregory* was call'd to the aſſiſtance of a woman in travail: The *fœtus* preſented in a tranſverſe poſition. He ſoon recovered the feet, and in a few moments delivered the woman.

The *funiculus umbilicalis* was ſo ſhort, that it was with difficulty he could make a ligature upon it, in order to make a ſeparation: He immediately extracted the ſecundine, and meaſur'd the *funiculus*, which was little more than four inches long.

As ſoon as the woman was taken care of, he examin'd the child, which he found to be imperfect in ſeveral parts, there being no *anus*, nor privities to diſtinguiſh of what ſex it was:

Where

Where the *vulva* ſhould be, there was a ſmall perforation (tho' no appearance of *labia*) thorough which the urine always paſs'd away: There was likewiſe a large *hernia umbilicalis*, and a little lower in the *linea alba* was a perforation, into which the *rectum* open'd; and that way the excrements paſs'd during the time the child liv'd, which was almoſt 10 weeks.

Several days before the child died, a gangrene appear'd on the *hernia*, which ſoon paſs'd into the inteſtines, and occaſion'd the child's death. The *hernia*, Mr. *Gregory* thinks, was occaſion'd by the ſhortneſs of the *funiculus*, which did not grow in length proportionable to the *fœtus*; the child was perfect in all other parts.

When the child died, Mr. *Gregory* only inſpected into the *rectum* (which he found as above deſcribed) and into the urinary bladder, which he found very ſmall, and no urine in it: The child was never obſerv'd to make water in a ſtream whilſt it liv'd, which makes him think the *ſphincter veſicæ* was imperfect.

In handling the bladder, he found ſomething ſharp pointing to his finger; he could not diſcover what it was till he ſnip'd off the neck of the bladder: He then took out of the bladder a tough kind of ſubſtance, about the bigneſs of a ſmall fig, in which was a pin with the head on, and very black.

A very extraordinary Calculus *taken out of the Bladder after Death; by the* Marquis de Caumont. Phil. Tranſ. N° 450. p. 369.

FIG. 4. Plate XII. repreſents an uncommon ſtone found in the bladder of a man after death, which the Marquis *de Caumont* had cauſe engrave in his own preſence, exactly conformable to the original.

The ableſt phyſicians and the beſt anatomiſts he conſulted on this ſubject, aſſur'd him they never ſaw any thing of the kind like it.

The engraving, tho' very exact, does not come up to this ſingular work of nature; its ten branches, which ſpread from the centre, have ſome reſemblance with thoſe of certain plants.

It is with him a matter of difficulty to think, that the ſyſtem of juxta-poſition, which is employ'd to explain the ſucceſſive growth of common ſtones or *calculi*, can hold good on this occaſion: He however, ventures not to advance that vegetation has any ſhare therein; tho' the ſhape of the branches of the ſtone, and of the canals or *papillæ*, which ſeem deſtin'd to convey

convey the nutritious juices, do in ſome meaſure favour this hypotheſis.

From the account of this ſtone ſkilful lithotomiſts may reap ſome advantage for perfecting their operations: For, allowing the poſſibility of *calculi* of a conformation ſomewhat like this, which they may judge of by knowing the bulk of the ſtone, they will underſtand, that in ſuch a caſe, no other method but that of the high operation can facilitate the extraction of an extraneous body, whoſe branches cannot fail cauſing conſiderable lacerations; unleſs they found ſome favourable circumſtances; and that the contexture of it were brittle enough to break it before its being extracted.

An Account of the above-mentioned Caſe; by M. Salien. Phil. Tranſ. N° 450. p. 371.

ONE *Joſeph Vaſſe*, of *Le Thor*, a ſmall town at a ſhort league's diſtance from *Liſle* in the County of *Venaiſſin*, 66 years of age, of a robuſt conſtitution, who uſed to travel about to fairs and markets in the ſaid County, and dealing in corn and cattle, without having ever complain'd of any indiſpoſition, began on the 14. of *February* 1731, to feel in the night-time ſome difficulty of making water, attended with a ſmarting about the *glans*; which, however, did not hinder him from attending his buſineſs as before.

On the 28th of *March* 1732, the ſaid *Vaſſe* was ſeiſed in the night with a true *iſchuria*, which cruelly tormented him.

On the 29th in the evening M. *Salien* was ſent for to ſearch him and to draw off the urine; he accordingly drew ſix cups, each containing a pint and a quarter: The patient found immediate eaſe, and continued without pain or fever; ſo that he thought himſelf entirely cured: But the night following the pains returned; which made him reſolve to come to *Liſle* to be nearer at hand to be ſounded. He came on the 30th of *March*; and had his water drawn off regularly every day, morning and evening till the 15th of *April* following, during all which time the patient ſuffer'd no pains, did not fall away, nor had any ſymptoms of ſickneſs upon him.

On the 15th of *April* he ſupped with his uſual appetite: But half an hour after ſupper he was ſeiſed with a violent ſhaking fit, which laſted a full hour; upon which a burning fever enſued, attended with an unquenchable thirſt, with a violent head-ach and extraordinary reſtleſsneſs.

In

In this condition M. *Salien* found the patient about 8 in the evening, being the hour he uſually went to ſound him. He immediately prepared himſelf to draw off the patient's water, according to cuſtom, thinking thereby to procure him ſome eaſe. Till then the catheter had enter'd without any obſtacle; but this time upon puſhing it into the bladder, he felt a ſtone which obſtructed its paſſage. He turned the catheter to the left, and hit upon one of the branches of the ſtone, as repreſented in Fig. 4. Plate XII.

In order to know whether there was not another ſtone, M. *Salien* drew the catheter a little back, turning it to the right, which was done without any difficulty: And having puſh'd it in again, he met with another branch of the ſame ſtone, which he took for a ſtone different from the former; and concluded then that he had found ſeveral ſtones in the patient's bladder: And that if the bad ſymptoms, which appear'd, ſhould continue any longer, there was no probability of his recovering.

Accordingly, the hiccough coming upon the patient on the 20th, and the other ſymptoms not diſcontinuing, he died on the 28th.

The ſtone was taken out four hours after his death.

The extraordinary figure of this ſtone will be of no great uſe in practical ſurgery; but it may furniſh matter of much reaſoning to philoſophers, to know how it could be formed in the bladder; and yet not be troubleſome to the patient for ſo long a time; and what had given it ſo particular a figure and ſo regular a ſhape.

M. *Salien*, for his part, queſtions not but it was ſuſpended in the patient's bladder, where it might be framed by the urine; the dried membranous filaments, which were ſtill perceivable on the extremities of ſome of the branches, are a new proof of this conjecture. The ſtone happening afterwards to looſen itſelf, may have occaſioned to the patient all thoſe ſymptoms that at laſt befel him, and afterwards death itſelf.

This opinion may appear extraordinary, to able lithotomiſts; and he ſubmits it to their better judgment.

Concerning the above-mentioned Stone; *by Sir* Hans Sloane. Phil. Tranſ. N° 450. P. 374.

THE above-mentioned ſtone is ſo ſingular, that among ſome hundreds of thoſe in Sir *Hans Sloane*'s poſſeſſion, he had not any, that came near it.

Once, it is true, he had under his care a Gentleman between 60 and 70 years of age, who had extraordinary difficulties in making water, and an inconveniency even beyond that; namely, that he could not ſit in an ordinary chair, without ſuffering exceedingly in the region of the *peritonæum*.

With the help of lenient ſoft medicines and waters, the patient voided by the *urethra* a ſtone, which was flat in the middle, and ſmooth, but had five points, like the rowel of a ſpur: The points of the rays were ſharp, but there were no aſperities nor cryſtallizations on their ſurfaces: The ſtone was ſmall, ſo as after many days to paſs along the *urethra*: But had it not paſſed thro' the neck of the bladder, but remain'd therein, it would, in all probability have attracted matter to all the points or rays, and increaſed in all dimenſions.

It is very common, that when any extraneous ſolid ſubſtance gets into the bladder, there is either attracted to it, or there adheres to and ſurrounds it, a tartareous calculous concretion, which aſſumes the figure of the ſaid body now in its centre, as a *nucleus*.

There was a ſoldier cut in St. *Thomas's Hoſpital, London*, for the ſtone, which, when extracted, was found to cover a muſket-bullet, that had been ſhot into his bladder, where it was cover'd by a calculous concretion.

A gentlewoman thinking with a ſilver bodkin to thruſt back a ſtone that was engaged in the neck of her bladder, it ſlipped into it, and the calculous matter gather'd on the larger end into a ſtone of an oblong figure and equal thickneſs, half an inch all round the bodkin.

A common pin, which by ſome means or other had got into the bladder of a young woman, was there coated all over by a calculous matter; but having occaſion'd a fiſtulous ulcer in her groin, it was diſcharged thence with the matter of the *fiſtula*.

It is in this manner that bezoars are formed: For, the Dr. has the common *Eaſt-India* bezoars, which are roundiſh, and have in their centres the ſeeds of a ſort of *acacia*, which had attracted, or was coated over by that ſubſtance, eſteemed a great cordial or alexipharmic; while others are long, and gather'd in layers or coats upon the ſtalks of vegetables: And he has one formed round the ſtone of that great plumb, which comes pickled from thence, and call'd *mango*.

As to the aſperities or prickles on the rays, they are taken notice of, ſo long ago as the time of *Cornelius Celſus*, who *lib.* VII. *c.* 26. calls them *calculi ſpinoſi*.

It

It may ſeem very ſtrange and paradoxical, that the fewer thy knobs, aſperities or prickles are on the ſurface of *calculi*, the more troubleſome they are to the perſons in whoſe bladders the lie.

Dr. *Hickes* was greatly tormented with the ſtone in his bladder, eſpecially upon any motion. He would not, on account of his age and ſeveral other reaſons, ſubmit to be cut for the diſtemper; but order'd his executors that he ſhould be open'd after his death, and the ſtone taken out of his bladder, put into a ſilver box, and given Sir *Hans Sloane* to be placed in his collection of ſuch kind of curioſities. What is very particular in this ſtone is, that the protuberances and prickles upon it were few, and at a diſtance from each other: Every one of them had made a hole in his bladder, like a ſheath or ſocket; and when upon motion they were remov'd out of their correſponding ſheath, they hurt the bladder in the ſound parts, and put him upon the rack.

When the prickles are thick-ſet, one hinders the other from entering or wounding ſo deep; and probably, does not get much farther than the *mucus* which lines the inſide of the bladder.

Some Oil of Saſſafras *cryſtalliz'd; by Mr.* Maud. Phil. Tranſ. N° 450. p. 378.

MR. *Maud* obſerv'd the following uncommon phenomenon in chemiſtry, namely that ſome eſſential oil of ſaſſafras, which had ſtood expos'd to a froſty night in an open veſſel, was changed, three parts out of four, into very beautiful tranſparent cryſtals, three or four inches in length, half an inch in thickneſs, and of an hexagonal form.

Theſe cryſtals ſubſided in water, were indiſſoluble in it, inflammable in the fire, and when expos'd thereto melted into their priſtine ſtate.

Hence it is evident, that they ſtill retain the natural qualities of an oil, tho' they appear under a different modification of their conſtituent parts.

What is moſt remarkable herein is a tranſmutation from a fluid to a ſolid body, of ſuch a particular figure, and from a yellowiſh liquor, not unlike *Madera* wine, to a very pellucid body, like ice, congeal'd from the moſt tranſparent water.

This ſeems to afford a new inſtance of cryſtallization, which being generally accounted for by the particles of a fluid, or thoſe of any other body, ſuſpended by the fluid, brought nearer by

by cold; and at length coming within the ſphere of each others attraction, unite together into an immediate contact.

This oil being one of the heavieſt oils, and even heavier than water, is the more likely thus to unite, as its parts are nearer to one another.

This may be a hint to the curious, to diſcover wherein conſiſts the difference of ſolidity and fluidity; and likewiſe ſhews how much the colour of bodies depends on the mechanical ſituation of their parts.

In *Phil. Tranſ.* N° 389. and 431 Dr. *Neuman* gives an account of a like cryſtallization from thyme, which he calls *camphora thymi*.

An extraordinary Damp in a Well *in the* Iſle *of* Wight; *by Mr.* Benjamin Cooke. Phil. Tranſ. N° 450. p. 379.

IN *June* 1733, a farmer, in hopes of finding a perpetual ſpring of good water, ſunk a well, whoſe diameter was ſeven to the depth of 45 feet (thro' a ſoil, whoſe ſurface was a kind of brick earth, mixt with ſand, which in deſcending became almoſt entirely hard coarſe yellow ſand) which work employ'd the labourers about 20 days, without finding the leaſt appearance of water.

At the diſtance of about 18 foot from the top, a *ſtratum* of a mineral mixture, about 9 inches thick, was dug thorough, without any inconveniency; nor were the workmen in the leaſt incommoded in carrying on the work, till about the 12th day after, when towards the evening they were much annoy'd with a faint ſuffocating heat, (which they compar'd to that coming from the mouth of an oven) and which, as they were drawn up, was moſt remarkably perceiv'd, when they came over againſt the mineral *ſtratum* abovementioned, to come out in the form of a warm ſulphureous *halitus*.

The next morning, a luſty young man attempted to go down (hand over hand, as they call it) by means of a ſingle rope which was uſed to draw up the earth dug up: But as ſoon as he came over againſt the above-mentioned *ſtratum*, he became incapable of ſuſtaining his own weight, fell down to the bottom, and died immediately.

Another young man, not ſuſpecting the cauſe, had the rope nimbly drawn up, and having ſeated himſelf aſtride a croſs ſtick, fixt to the rope for that purpoſe, was haſtily let down to his friend's aſſiſtance: But when he came to the ſame diſtance from the top, he was obſerv'd to give the rope a very great

ſhock;

ſhock; and when he came to the bottom, fell down, as the other had done before him, was ſeized with violent convulſions, which held him more than a quarter of an hour, and then he expired.

A third perſon in hopes of fetching up this ſecond before he was quite dead, was tied faſt into a large baſket, and let down more warily: But when he came to the ſame *ſtratum*, finding his breath going (as he expreſs'd it) he cried out, and was drawn up again; but remain'd in the open air, for the ſpace of near half an hour, pale as dead, panting and ſpeechleſs.

The dead bodies were, within three hours time, drawn up by means of a ſort of tongs, uſed to fetch things up from the bottom of the ſea; but brought ſuch a diſagreeable ſtench in their cloaths with them, as made ſeveral hardy men, who aſſiſted in doing of it, vomit.

The next day a cat was let down, and at the ſame place ſeized with convulſions: But being drawn quickly up again, ſoon came to herſelf; which experiment was repeated ſeveral times for ſome weeks following; whereby it was found, that this deſtructive vapour was ſometimes of a greater, and ſometimes leſſer force, and at other times quite gone; ſo that the cat felt no uneaſineſs: And a lighted candle, which would ſometimes be immediately extinguiſhed, as ſoon as it ſunk below this deadly *ſtratum*, would burn clearly at the very bottom.

It was very remarkable, that there was a whitiſh fog in the well, ſo thick that one could but juſt ſee the dead bodies through it.

Water being ſcarce in that place, the well was left open for about eight months, in hopes the damp might at length entirely leave it; but inſtead of that, it became worſe; and not confining itſelf within its firſt bounds, it overflow'd at the top; where when the air was moiſt, it appear'd like a thin white fog; and when the air was dry, could be perceiv'd like a warm breath, at all times diffuſing a ſulphureous ſtench (ſomething like that which ariſes from filings of iron while corroding with vinegar) affecting thoſe who came into it with a giddineſs, ſhortneſs of breath, and propenſity to vomit: So that at laſt the well was fill'd up, being troubleſome to the family which liv'd near it.

The above-mentioned *ſtratum* is continued to the neighbouring clift; where when heated with the ſummer's ſun, it gives a noiſome ſulphureous ſtench; and, after moderate rains, is covered with

with a yellowiſh efflorescent ſalt, very aſtringent and acid. On the ſhore below are gather'd *pyrites*.

By a letter afterwards from Mr. *Cooke*, he ſays, that the vein which was cut through the middle of the well, from whence were emitted the fatal effluvia, is a crude ore made up of iron, ſulphur, and acid ſalts, mixt with *pyrites*.

Theſe effluvia were not perceiv'd till after the vein had imbib'd the air for ſeveral days.

Whilſt the air continued dry, theſe effluvia ſubſided, and lay in the lower part of the well, which ſeem'd fill'd near to an exact level with the *ſtratum* from whence they came.

But when the weather became rainy, the quantity as well as the *impetus* of the effluvia increaſed to ſuch a degree, as to appear in mornings over the top of the well, in the form of a miſt, and gave great annoyance to thoſe that came within its ſphere of activity.

From hence it is worth obſerving, that the ſame damp, according to the variation of the weather, is ſpecifically heavier or lighter than the air.

Concerning Magnets *having more* Poles *than two*; *by Mr.* John Eames. Phil. Tranſ. N° 450. p. 383.

THE ſagacious Dr. *Halley*, in his account of the changes of the variation of the magnetical needle, upon the hypotheſis of the earth's being one great magnet having four magnetical poles, tells us, that he had found two difficulties not eaſy to ſurmount; the one was, that no magnet he had ever ſeen or heard of, had more than two oppoſite poles: Whereas the earth had viſibly four, if not more, &c.

In looking over the copy of the journal-book of the *Royal Society*, Vol. II. Mr. *Eames* finds the following article.

'*July* 20, 1664. Mr. *Ball* produced ſeveral loadſtones, and 'among them two *terrella*'s, one of which ſeem'd to have four 'poles, with a circle paſſing between them, of no virtue at all. 'Some of the company ſuggeſted, that it was probable this 'ſtone conſiſted of two ſtones, by nature cemented together 'by a piece that had no magnetical quality in it.'

Qu. Whether this ſtone can be come at, to examine whether it be a ſingle or a double ſtone?

An

An Account of some Magnetical Experiments; by Dr. Desaguliers. Phil. Trans. N° 450. p. 314.

IN 1715, trying some experiments on a very large weak loadstone, Dr. *Desaguliers* found that it had several poles: Then he tried several other loadstones, and often found four poles in such as had been armed, when he took off their armour.

In large coarse stones he found sometimes eight, nine or ten poles: This made him believe all loadstones to have several poles: But when he tried My Lord *Paisley*'s loadstones, and other very good ones, he then found that homogeneous loadstones had but two poles; those that have more being only an aggregate of magnetical and other matter, which makes an heterogeneous substance. Such is the *Society*'s great loadstone: For, it has several poles.

On the 24th of *June* 1736, the Dr. made the following experiment, before the *Royal Society*.

He took a bar of iron, of one fourth of an inch in diameter (which having stood for 15 years in an erect position, had acquir'd a fixt pole at top: So that the end, which had stood uppermost, attracted the north end of a compass needle, and the other end the south end of the needle) and having suspended it by a string for the space of half a year, it acquir'd a fixt south pole at that end, as well as it had done at the other in 15 years time, without diminishing the virtue of the other end; So that both ends of the rod in any situation attracted the north end of the compass needle.

That rods of iron untouch'd, or which have not acquir'd a magnetic virtue by their situation, will with their upper end (whatever end of the bar be held upwards) attract the north end of the needle; and the lower end of the bar the south end of the needle, is a truth known many years ago, and mentioned in Dr. *Brown*'s book of *Vulgar errors*.

Some farther Magnetical Experiments made before the Royal Society April 21, 1737; *by the Same.* Phil. Trans. N° 450. p. 386.

IT is well known, and has been often found by experience, that an iron bar, untouch'd by a loadstone, will, with its upper end, attract the north end of a compass needle, when the said bar is held upright, and the south end of the needle with its lower end, when applied to it, still in a perpendicular position, whatever end of the bar be held upwards; unless the bar

bar has acquir'd a fixt pole by having been long in a vertical position. But if the bar be brought from a vertical to an horizontal position, the needle will return into the situation it had before, which was in the magnetical meridian, the bar being then at right angles to it. Upon raising or sinking the end of the bar which is farthest from the needle, the one or the other end of the needle will begin to move towards the bar: Such a bar has in itself no fixt magnetic virtue; but if it had, it must be heated red-hot, and then cool'd in an horizontal position: A bar thus prepared is fit to make the following experiments, communicated to the Dr. by M. *Du Fay*.

Hold the bar upright, and give it a blow or two against the ground with its lower end; and that end will attract the south end of the needle, when the bar is held horizontal, and at right angles to the magnetic meridian: The other end, held horizontal in the same manner, will attract the north end of the needle. Invert the bar, and its virtue will be lost by striking as many blows with it against the ground with the other end: Then strike another blow or two; and the end, which attracted the north end of the needle, will now attract the south end; and so *vice versâ*, the position being still horizontal.

If the blow be given against the ceiling, or any horizontal body, with the upper end of the bar, the same virtue will be communicated as before.

This will likewise happen, if the upper or lower end of the bar be struck with a hammer or mallet; whether the blow be given end-wise, or at right angles to the bar: Nay, tho' it should be given in the middle of the bar, the position of the bar at receiving the blow being all that is requisite: For, if you give the bar only a jerk or shake in that vertical position, it will receive the virtue, as if there were in the iron several threads, or beards, fixt at one end, as M. *Du Fay* supposes, which the blow or shake laid all one way, and which were placed the other way by inverting the bar; and then giving it a shake or blow.

N. B. When the bar is placed horizontally, a blow in the middle destroys its virtue.

An

An antique Metal Stamp in the Duke of Richmond's *Collection, being one of the Instances how near the* Romans *had arriv'd to the Art of* Printing; *with some Remarks by Dr.* Mortimer. Phil. Trans. N° 450, p. 388.

SINCE arts and sciences, especially statuary and sculpture, were arriv'd at so great perfection, when the *Roman* empire was in its glory; as the many beautiful statues, the exquisite *intaglia*'s, and fine medals, which time hath handed down to us, do sufficiently evince: It is very surprising, that they never hit on the method of printing books.

The dies they made for their coins, and their stamping them on the metal, was in reality printing on metal; their seals cut in cornelians and agates, and their stamping them on dough and soft wax, was another sort of printing; and a third sort was the marking their earthen vessels, while the clay was soft with the name of the potter, or the owner the vessel was made for. These being of a larger size were properly call'd *signa*; the seals cut in stone were call'd *sigilla*, being a diminutive of *signum*: But the later and more barbarous *Latinists* have formed the diminutive of *signum* into *signetum*; and if a very small pocket seal, they have call'd it *signaculum*. Vide *Joh. Mich. Heinecius de sigillis. Francof.* 1709 fol. p. 16 & *seq.*

The learned *Montfaucon*, amongst his prodigious treasures of antiquities, in his *Antiquité expliquée* Tom. III. Part 2, ch. 12. gives us the figures and descriptions of several of these larger *sigilla*, or *signa*, on which, *says he*, the names were all cut in *creux*, in capital letters; he imagines their use to have been to mark earthen vessels, particularly those large earthen jars, wherein the *Romans* used to keep their wines.

If any of them had occurr'd to him with the letters *excisæ*, *exculptæ*, protuberant or standing out, as the types in our modern way of printing are made, so accurate a describer of antiquities could not have pass'd such a one over without taking notice of it; and that the rather because of its being a greater rarity: Tho' several lamps of *terra cocta* are stamp'd with letters impress'd or hollow, from such protuberant letters as in Fig. 5. Plate XII. but the greater number have the letters rais'd or standing out.

The said Fig. represents one of that sort of stamps, whereon the letters are *exsculptæ* or protuberant; as is likewise the edge or border round the whole stamp.

This

This ſtamp is made of the true ancient braſs, and is cover'd over with a green ſcale or coat, ſuch as is uſually ſeen on ancient medals. It was found in or near *Rome*. On the back is faſtened a ring, the hole whereof is $\frac{7}{40}$ of an *Engliſh* inch one way, and $\frac{23}{40}$ the other way: The plate itſelf is two inches long, wanting $\frac{1}{40}$, and its breadth exactly $\frac{33}{40}$ of an inch: The ſides are parallel to one another, and the ends are likewiſe parallel to each other; but they are not upon an exact ſquare with the ſides, varying about one degree and a half from an exact rectangle. On the under ſide ſtand two lines or rows of letters $\frac{7}{40}$ of an inch in height, and well formed *Roman* capitals: The faces of them ſtand up all upon an exact level with one another, and with the edge or border of the ſtamp; their protuberance or height above the ground is different, the ground being cut uneven: For, cloſe to moſt of the letters, the ground is cut away only $\frac{2}{40}$; cloſe to ſome, near $\frac{3}{40}$, and cloſe to the edges, full $\frac{3}{40}$.

The firſt line of the figure contains the following letters CICAECILIV, with a ſtop or leaf to fill up the line; in the ſecond line HERMIAE. SN. which the Dr. judges is to be read *Caii Julii Cæcili Hermiæ Signum.*

Who this *Caius Julius Cæcilius* was, the Dr. cannot find, he being probably a man in a private ſtation; and ſo his name hath not been handed down to us in any monuments, but only accidentally in this ſtamp.

In *Gruter* occur two of the name of *Hermias*, and ſeveral of the *Cæcilii*, but none with theſe two names joined together.

The uſe of this ſtamp ſeems to have been for the ſignature of the above-mentioned private man, to ſave him the trouble of writing his name, as ſome people have now adays. It was certainly uſed on paper or membranes, being firſt dipt into ink, or ſome ſort of paint, becauſe of the protuberance of the letters; the hollow letters being fitter for ſoft ſubſtances, on which they leave the impreſſion ſtanding up, and conſequently more legible.

Another argument, that this ſtamp was not to be uſed on any ſoft ſubſtance into which it might be preſs'd quite down to the ground, is the unevenneſs or roughneſs with which the ground is finiſh'd; which, was it to have made part of the impreſſion, the workman would have finiſh'd with more accuracy; but he knowing that the ſurface of the letters was to perform the whole work requir'd, was only attentive to finiſh them with that accurate evenneſs that theſe have.

The

The learned M. *Mattaire*, in his *Annales typographici Hagæ*, 1719 in 4*to* p. 4, concludes from the beſt authors, that our modern art of printing was firſt thought of about 1440, a copy of the book he mentions *ibid. p.* 13, call'd *ſpeculum noſtræ ſalutis*, being pictures of ſtories out of the bible, with verſes underneath in *Dutch*, which the Dr. has ſeen in the *Stadt-houſe* at *Harlem*: Each page was printed from a block of wood, like a ſorry wooden cut: And this was the firſt eſſay of printing, which hint was taken from engraving, and is what he means p. 4. by *typi fixi*: After which they ſoon improv'd to uſe ſeparate types, as we now do, which he terms *ibid. typi mobiles.*

This ſtamp is in reality a ſmall frame of fixt types, and prints with our modern printers ink, which is only a ſort of black paint, as readily as any ſet of letters, cut in the rude manner theſe are, can be expected to perform: The Dr. has, therefore, given the figure of it, as the moſt ancient ſample of printing known of: For, by the appearance of the metal it ſeems to be of the upper empire.

By this ſtamp it appears, that the very eſſence of printing was known to the *Romans*; and they had nothing to do but to have made a ſtamp with lines three or four times as long, and containing twenty inſtead of two lines, to have formed a frame of types, that would have printed a whole page, as well as *Coſter*'s wooden blocks, which he uſed in printing the *ſpeculum ſalutis.*

In the firſt volume of a collection of ſeveral pieces of Mr. *John Toland*, printed *London* 1726, 8vo. p. 297. is a ſmall tract of his, intitul'd *Conjectura veroſimilis de prima typographiæ inventione*, which is founded upon the following paſſage in *Cicero cap.* 20. *lib.* ii. *de natura deorum*; where *Balbus* the ſtoic uſes the following words in an argument againſt *Velleius* an *Epicurean.*

'Hic ego non mirer eſſe aliquem, qui ſibi perſuadeat, corpora quædam ſolida atque individua vi & gravitate ferri; mundumque effici ornatiſſimum & pulcherrimum, ex eorum concurſione fortuita? Hoc qui exiſtimet fieri potuiſſe, non intelligo cur non idem putet, ſi innumerabiles unius & viginti formæ literarum (vel aureæ vel quales libet) aliquo conjiciantur; poſſe ex his in terram excuſſis annales Ennii, ut deinceps legi poſſint, effici; quod neſcio anne in uno quidem verſu poſſit tantum valere fortuna.'

He conjectures that this very paſſage gave the firſt hint to the inventors of printing about the year 1445; becauſe they retain'd

retain'd even *Cicero*'s name for their types, calling them *formæ literarum*, and made them of metal, as he ſays, *aureæ vel quales libet*. Moreover in cap. 10. lib. III. *de Divinatione*, *Cicero* hath the very phraſe *imprimere literas*.

Brands for marking cattle were in uſe in *Virgil*'s time, *Georg. lib.* III. *ver.* 158, where he ſays,

Continuoque notas, & nomina gentis inurunt.

Procopius in his *Hiſt. Arcana* ſays, that the Emperor *Juſtinus*, not being able to write his name, had a thin ſmooth piece of board, thro' which were cut holes in form of the four letters repreſented in Fig. 6. Plate XII. which, laid on the paper, ſerv'd to direct the point of his pen, which being dipt in red ink, and put in his hand, it was directed by another.

Poſſibly this may likewiſe have given the hint to the firſt of our card-makers, who paint their cards in the ſame manner, by plates of pewter or copper, or only paſt-boards, with ſlits in them in form of the figures that are to be painted on the cards.

An Occultation of Mercury *by* Venus May 17, 1737 *at the* Royal Obſervatory *at* Greenwich; *by Dr.* Bevis. Phil. Tranſ. N° 450. p. 394. *Tranſlated from the* Latin.

App. Time p. m. H.	M.	S.	
1	37	3	The preceding limb of *Venus* paſſes the meridian, the centre being 25° 46′ 35″ diſtant from the vertex.
			But the Dr. did not at all ſee *Mercury* in the teleſcope.
9	4	9	The centre of *Mercury* preceded the preceding limb of *Venus* 12″ of time.
	6	20	The obſervation repeated, when the centre preceded the ſame time as before.
	28	0	*Mercury* running along the parallel thread of the micrometer, the ſouthern cuſp of *Venus* was cut by the ſaid thread (Fig. 7. Plate XII.) whence the Dr. gather'd that *Venus* would cover *Mercury* or at leaſt juſt touch him: He, therefore, remov'd the micrometer, the better to obſerve the approaching contact with a teleſcope of 24 foot.

App.

App. Time p. m.
H. M. S.

9 43 4 *Mercury* is but the tenth or twelfth part of *Venus*'s diameter distant from her: Afterwards clouds intercepted the view.

51 10 *Venus* shone out again very bright; and *Mercury* was entirely hid under her: Now clouds again cover *Venus*, and intercept the view of so uncommon a sight.

May 18. P. M.

The meridian distance of the sun from the vertex was 30° 4′.

1 31 53 The preceeding limb of *Venus* passes the meridian. The centre distant from the *vertex* 25° 57′ 15″.

The Dr. could neither this day see *Mercury* culminate, tho' the sky was very serene.

N. B. The distances from the vertex are not free from refractions.

A new azimuth Compass *for finding the Variation of the* Compass-needle, *at Sea, with greater ease and exactness than by any hitherto contriv'd for that purpose; by* Capt. Christopher Middleton. Phil. Trans. N° 450. p. 395.

TO discover the declination of the magnetic needle, or variation of the compass at sea, with some tolerable degree of certainty and exactness, is a thing of great use and importance in navigation.

The instruments and methods hitherto made use of for this purpose (as might easily be demonstrated) are subject to several inconveniencies, errors and defects: To remedy which, this new azimuth compass was contriv'd, and has by experience been found effectual. It would be needless to give a description of the instrument, the Capt. therefore, only shews the manner of using it, which is, as follows.

1. The instrument must be rectified, or fitted for observation, by turning it about till the 4 cardinal points, that are suspended upon the centre-pin, agree with the 4 cardinal points on the chart, at the bottom of the box: Then will the needle, that shews the magnetic meridian, stand at no degrees; and the east and west points at 90 degrees, on the graduated circle within the

the box: And in this ſituation it muſt be kept, as near as may be, during the whole time of the obſervation.

2. Let the index of the quadrant be placed to that degree of the arch, on the rim of the box, which the obſerver judges to be nearly equal to the height of the ſun, or ſtar, whoſe azimuth is ſought: For, by this means the object will be more readily found.

3. Turn the quadrant round towards the ſun or ſtar, till it appear upon the vertical hair within the teleſcope, to an eye looking thorough the ſmall hole or ſight; and then ſlide the index a little upwards or downwards on the arch, till the object be brought by this means to coincide or touch the viſible horizon.

Laſtly, the degrees and minutes then markt by the index upon the arch of the quadrant, will ſhew the altitude of the object, which will always be the ſame, whether the inſtrument is in motion or at reſt; at the ſame time the degree cut by the index on the horizontal rim or circumference of the compaſs-box, will give the magnetical azimuth of the ſun or ſtar.

N. B. All this may be performed by one perſon: Whereas the old compaſs requires ſeveral to manage it; which likewiſe makes it ſubject to ſeveral conſiderable errors.

How the variation of the needle is found by means of magnetical azimuth and altitude thus obtain'd, is taught in every treatiſe of navigation: But as the reſolution of this problem is ſomewhat troubleſome, and requires ſuch a knowledge of the doctrine of the ſphere, as every ſeaman has not attain'd, the Capt. here exhibits an eaſy method of diſcovering the variation of the compaſs without any manner of calculation.

1. Let the magnetic azimuth of the ſun (or any ſtar, when it is near the prime vertical, and conſiderably elevated above the horizon) be found according to the directions already given, before it arrive at the meridian; and note well the altitude, or let the index remain fixt at the ſame point on the arch.

2. Find the magnetic azimuth of the ſun or ſtar in like manner as before, when it is exactly at the ſame degree of altitude, after it has paſs'd the meridian: And

3. If theſe two magnetical azimuths are equal, the needle has no variation; if unequal, add them together, and half the ſum will be the true azimuth; or ſubſtract the leſs from the greater, and half the difference will be the variation requir'd. The circumſtances of the obſervation will the more readily diſcover, whether the declination is eaſterly or weſterly.

N. B.

N. B. Tho' it would be very commendable in ſuch as uſe the ſea, to learn the names of moſt of the principal fix'd ſtars; yet even that knowledge is not neceſſary in the uſe of this inſtrument: Nor is it needful in this caſe to know exactly the latitude of the place of obſervation, provided the difference of latitude between the obſervations be not very great. It is ſufficient that care be taken to obſerve the ſame ſtar, before it comes to the meridian, and after it has paſs'd it: And for the ſake of greater exactneſs, the above caution ſhould be regarded, to wit, that the ſtar be at ſome conſiderable height above the horizon, and likewiſe near the prime vertical.

The Number of People in Holland *and* Weſt-Frieſland, *as alſo in* Harlem, Gouda *and the* Hague, *drawn from the Bills of Births, Burials, or Marriages in thoſe Places; by M.* Kerſſeboom. Phil. Tranſ. N° 450. p. 401.

EVERY body knows to what uſeful purpoſes the bills of births and burials at the City of *Breſlau*, the Capital of *Sileſia*, have been applied by Dr. *Halley*; as alſo what curious obſervations both moral, phyſical and political have been made by Sir *William Petty* upon the ſame argument, ſeveral years before and by Dr. *Arbuthnot* and others ſince. M. *Kerſſeboom* hath not only conſulted them, but acquainted himſelf more particularly with Mr. *King*'s obſervations in *Davenant*'s eſſays, *&c.* in order to render himſelf more capable of making a juſt eſtimate in this matter.

He begins with the number of inhabitants in the two provinces of *Holland* and *Weſt-Frieſland*; theſe he makes at this time, *viz.* in 1738, to amount in all to 980,000, and preſents the reader with the following table of the particulars.

It exhibits the number of people of all ages, living at the ſame time, from the birth to extreme old age; which becauſe it ſhews the chances of mortality within the ages mention'd, he calls the table of contingency of life and death.

The table of contingency

Of above	90 years of age there are			500
	90	86		2,500
	85	81		6,500
	80	76		13,000
	75	71		20,300
	70	66		27,300
	65	61		34,300
	60 to	56	inclusive	40,800
	55	51		47,000
	50	46		53,000
	45	41		57,800
	40	36		62,500
	35	31		67,600
	30	27		58,400
			The ſum above 27 years	491,500
Of	26	21		94,300
	20 to	16		83,400
	15	11		87,200
	10	6		91,800
	5 to	birth		131,800
			Sum under 27 years of age	488,500
				491,500
				488,500
			Sum of all the inhabitants	980,000

This table is founded upon three principles, *viz.* correct obſervations upon the tables of aſſignable annuities in *Holland*, which have been kept there upwards of 125 years; wherein the ages of the perſons dying are truly enter'd: Upon a ſuppoſition that there are yearly born in the two provinces 28.000 living children; and laſtly, that the entire number of inhabitants in any country is to the number of the births as 35 to 1.

This table was ſent ſome time after its compoſure to Profeſſor *S'Graveſande* to know his thoughts, as well concerning its juſtneſs, as its fitneſs to aſcertain the value of annuities on lives; and it met with his approbation.

From

From this table it appears

1. That about half the number of people in the two provinces are above 27 years of age, and consequently that near the other half are under that age.

2. Then by following what hath been obsery'd for more than 100 years in *England*, and particularly in *London*, out of 35 children born, 18 of them are boys, and 17 girls, the people in these two provinces will consist of

504,000 Males.
476,000 Females.

980,000

He farther remarks, that it appears from the assignable annuities for lives mentioned before, the females have in all accidents of age liv'd about three or four years longer than the same number of males; which he looks upon to be appointed as a compensation for the continual excess there is in the birth of the males above that of the females.

Having considered the quantity, he then comes to take notice of the quality of these 980,000 inhabitants, and says he sees no reason to differ from the proportion of Mr. *King* in *Davenant*'s essays, who with a great deal of pains and judgment hath divided the people of *England* in the following manner.

The proportion for every 100,000 inhabitants is

Married men and women	34,500
Widowers	1,500
Unmarried young men and children	45,000
Servants	10,500
Travellers, strangers, &c.	4,000
	100,000

If this proportion be admitted, then the number of each sort in *Holland* and *West Friesland* will be as underneath.

M. *Kersseboom* adds that the said provinces can raise at this time 220,000 able bodied men, deducting one tenth for diseases and other infirmities: But then he admits persons at 16 years of age; whereas Dr. *Halley* admits none till 18; persons

ſons under that age being generally too weak to bear the fatigues of war, and the weight of arms.

He then proceeds to rectify the miſtakes of the learned *Iſaac Voſſius*, who makes but 550,000 in *Holland*, *Weſt-Frieſland*, &c. Diſallows Sir *William Petty*'s account of the number of people in *London*; becauſe he makes them alone equal to the inhabitants of *Holland* and *Weſt-Frieſland* together.

He cloſes the whole with a table of the preſent value of annuities upon lives, in proportion to the ordinary or common bonds, charged upon theſe provinces, and ſubject to the extraordinary taxes rais'd at this time, *viz.* 1738.

Hereto is annex'd the degrees of mortality or fatality ſaid to be in the *Hague* and *Haagambagt*, as alſo the numbers and conditions of the inhabitants of *Amſterdam*, *Harlem*, *Gouda*, and the *Hague*, not omitting *London* at this preſent time.

The two provinces of *Holland* & *Weſt-Frieſ.*		*Amſter-dam*	*Har-lem.*	*Gouda.*	*Hague*	*London*
Married men and women	338000	86156	17420	6900	14850	241800
Widowers	14700	4218	760	300	720	13100
Widows	44100	13858	2280	900	2380	45700
Unmar. youth and children.	441000	93990	22700	9000	16190	215700
Servants	102900	28318	5300	2100	4870	85000
Travellers ſtrangers, &c.	39300	14460	2040	800	2490	52300
Total	980000	241000	50500	20000	41500	653600

The fatality of the quarters.	Dead.
Spring to ſummer	307
Summer to the autumnal equinox	286
Autumn to winter	287
Winter to ſpring	286

The

The fatality of the months 31 years, one with another.

	Dead.
January	102
February	88
March	95
April	77
May	112
June	100
July	92
Auguſt	95
September	99
October	93
November	95
December	99

Hence it appears that *March* is leſs fatal at the *Hague* and *Haagambagt* than *April*; and *April* than *May* and *June*; that *May* is the moſt fatal month of all; that the remaining months are nearly equal.

It appears farther, that three parts or ſeaſons of the year are very nearly equal; but that the other quarter or ſeaſon, beginning at the vernal equinox, is more fatal than any of the reſt by one fifteenth part.

Table of annuities for life.

Let the annuity be 100 guilders a year, upon a life under a year old.

	Guild.	Guild. Stiv.
Its preſent value is	1667	that is 6 0 *p. cent.*
Upon a life of 5 years to 1 inclusive	1869	5 7
10 6	1835	5 9
15 11	1770	5 13
20 16	1667	6 0
25 21	1587	6 6
30 26	1515	6 12
35 31	1429	7 0
40 36	1334	7 10
45 41	1212	8 5
50 46	1093	9 3
55 51	971	10 6
60 56	840	11 8
65 61	709	14 2
70 66	570	17 11

Uſe

USE.

Queſtion. Let it be deſired to know the preſent value of any annuity for life, for inſtance, of 90 guilders a year, which was granted in the year 1703, upon a life then of three years old.

Anſwer. The life now in 1738 is between 37 and 38 years old: Hence the number between 40 and 36 gives 1334 for the preſent value of an annuity of 100 guilders: Hence $\frac{1334 \times 90}{100} = 1200$ guilders is the preſent value of the annuity for that life.

As to that part which treats of the number of inhabitants of *London* Mr. *Maitland* makes the following remarks.

M. *Kerſſeboom* aſſerts that the city of *Paris* in 1684, and at the cloſe of the laſt century, contain'd more inhabitants than the city of *London*.

And to prove that *Paris* contains a greater number of inhabitants than *London*, he has had recourſe to the accounts of chriſtenings annually publiſh'd in both cities, without giving himſelf the trouble to enquire into the nature of thoſe his authorities; which if he had done, he would ſoon have diſcover'd, that the former is a perfect account; while the latter is, perhaps, the moſt defective of any extant: For, the chriſtenings therein mentioned are only thoſe at which the pariſh clerks are preſent; which Mr. *Maitland* is of opinion cannot amount to near two thirds of the whole, as he endeavours to make appear.

The burials in the annex'd table, by ſome typographical errors in Mr. *Maitland's Political Account of his Hiſtory of London* p. 535, from which it is taken, being increaſed 491 above the real number in *Graunt's* account, *Nat.* and *Polit. Obſerv.* 3d edit. *Lond.* the ſum total whereof amounting to 90350, muſt be reduced to 89859: And as in the annex'd term of years, there appears to have died of the plague 1741 *Maitland's Hiſt. Lond.* p. 535. three and a half of which, Mr. *Maitland* computes would have died of common diſtempers, out of each hundred; which amounting to about 61, the ſame deducted from 89859, the real number of the burials, the ſum will be reduced to 89798, which taken from 90883, the total of the chriſtenings, the remaining ſum will be 1085, which divided by ten, the medium thereof will be 108 $\frac{1}{2}$ yearly in favour of the chriſtenings.

A de-

A decenary account of the chriſtenings and burials of *London*, in the following years.

Years.	Chriſten'd.	Buried. Com. Diſt.	Buried Plague.	Totals buried.
1626	6701	7400	134	7534
1627	8408	7713	4	7717
1628	8564	7740	3	7743
1629	9901	8771	0	8771
1630	9315	9228	1317	10545
1631	8524	8288	274	8562
1632	9584	9527	8	9535
1633	9997	8392	0	8392
1634	9855	10899	1	10900
1635	10034	10651	0	10651
To. Gen.	90883	88609	1741	90350

This difference in favour of the chriſtenings is owing to the citizens of that time being almoſt of the ſame perſuaſion: But the civil war breaking out ſoon after, the people ſeparated into a variety of ſects, ſubverted the Church of *England*, and aſſuming the civil power, eſtabliſh'd a new Church Government. But the members of the aboliſh'd Church continuing to baptize among themſelves (without reporting their chriſtenings to the new appointed members of the company of pariſh-clerks) occaſioned a very great defect in the account of chriſtenings, annually publiſh'd by the ſaid pariſh-clerks.

From this *epocha* is to be dated the excels of the burials in the bills of mortality over the chriſtenings of *London*. And tho' the Church of *England* was ſoon after re-eſtabliſh'd, yet the numerous diſſenters of all denominations, preſevering in their ſeparation, continued to baptize within themſelves, without ſending in accounts of their chriſtenings to the reſtor'd members of the company of pariſh-clerks: And the ſchiſm ſtill continuing, the accounts of the chriſtenings and burials of this city, remain upon the ancient foot of diviſion and imperfection.

Add to this, that not only all the foreign Churches in *London* chriſten within themſelves, but likewiſe ſeveral churches and chappels of the Church of *England*, that do not ſend in their accounts to the company of pariſh-clerks; which, together with thoſe of the diſſenters and foreigners of all denominations, amount to no leſs a number than one hundred and eighty one congregations, whoſe accounts of chriſtenings are

not

not publiſhed. By which it plainly appears, that the vaſt diſparity between the chriſtenings and burials of this city, is not owing, as M. *Kerſſeboom* vainly imagines, to the reſidence of the court, convention of parliament, and great reſort of people from all parts, but in fact to the great defect above mentioned.

However, from the aforeſaid very defective account of the chriſtenings of this city, M. *Kerſſeboom* has calculated the number of its inhabitants by a medium of the chriſtenings in the years 1684 and 1685; whereby he makes the number at that time amount to five hundred thouſand three hundred and fortie four: But as this number is only taken from a medium of two years, he imagines it too great; therefore to reduce the ſame to the number of four hundred and ſixty nine thouſand ſeven hundred, by a medium of twenty years, he has unwarrantably precluded the ſum of fourteen thouſand ſeven hundred and two, the number of chriſtenings in the year 1684, to make room for the ſum of eleven thouſand eight hundred and fifty one, the number of chriſtenings in the year 1674; whereby the number of inhabitants in *London*, is very much leſſened.

And as a farther inſtance of M. *Kerſſeboom*'s partiality in favour of the city of *Paris*, he has calculated the number of its inhabitants (without mentioning the uncertainty of a calculation, founded on a ſhort ſpace of time, as he has done in the caſe of *London*) at a medium of the chriſtenings for the years 1670, 1671 and 1672; whereby, he makes them at that time amount to ſix hundred and ten thouſand three hundred; adding, the number muſt have been greater at the end of the laſt century; as by his extravagant manner of calculation it ſhould be at preſent.

But as it appears by the above ſpecified ten years account, that the chriſtenings of *London* greatly exceed the burials of that time. Mr. *Maitland* thinks it will not be denied, that they exceed the ſame at preſent; eſpecially if we conſider, that the number of chriſtenings in *Paris*, at a medium of nine years (preceding that of 1737) exceeding that of the burials ninety eight yearly; notwithſtanding that city not only abounds with a vaſt number of religious of both ſexes, who are ſworn to celibacy, but likewiſe many thouſand ſtudents belonging to the univerſity, who lead a ſingle life: Whereas in *London* there are no ſuch perſons to prevent the increaſe of its inhabitants.

And as in Mr. *Maitland*'s political account of *London* p. 540. and 548. it appears, that at a medium of nine years, there are annually buried in *London* 29542, and in *Paris* only 17804, which

which is 11738 in favour of the former. So muſt the births in *London* at preſent (according to the above-ſpecified ten years account, the reaſons aforeſaid, and the *Paris* account of chriſtenings) yearly exceed thoſe of *Paris* 12320; whereby is ſhewn, that the inhabitants of *London* exceed thoſe of *Paris* above three fifths in number.

What M. *Kerſſeboom*'s partiality in favour of the city of *Paris* is owing to, Mr. *Maitland* does not know, unleſs it be out of pique to Sir *William Petty* (with whom he ſeems not well pleas'd) for ſaying, that the city of *London* contain'd as many inhabitants as the province of *Holland* and *Weſt-Frieſland*: Which Mr. *Maitland* thinks will be no difficult matter to make appear, by allowing M. *Kerſſeboom*'s ſuppos'd number of 28000 children to be annually born in the ſaid province: Whereas according to the above-ſpecified ten years account, and the *Paris* proportion of births, there muſt be annually born in *London* thirty one thouſand and eight children.

Therefore as this number, according to Mr. *Maitland*'s calculation *Hiſt. Lond.* p. 541, is the produce of 725903, the preſent number of the inhabitants of *London*, ſo muſt 28000, the number of children ſuppos'd to be born yearly in the province of *Holland* and *Weſt-Frieſland*, be the produce of 655485, the preſent number of the inhabitants of the ſaid province. Notwithſtanding M. *Kerſſeboom* by his exceſſive and unprecedented reckoning of the births at a thirty fifth part of the people, has calculated them at 980000. Whereas by the ingenious and learned Dr. *Halley*'s method of calculation (which is ſo highly approv'd of by M. *Kerſſeboom*, that he ſeemingly would be thought to make it the ſtandard of his calculations) the inhabitants of the province of *Holland* and *Weſt-Frieſland* do not amount to twenty nine times the number of the births; which gives room to ſuſpect, that M. *Kerſſeboom* has introduced this unheard of exceſs, to increaſe the number of people in the ſaid province of *Holland* and *Weſt-Frieſland*.

The END of the TENTH and LAST VOLUME.

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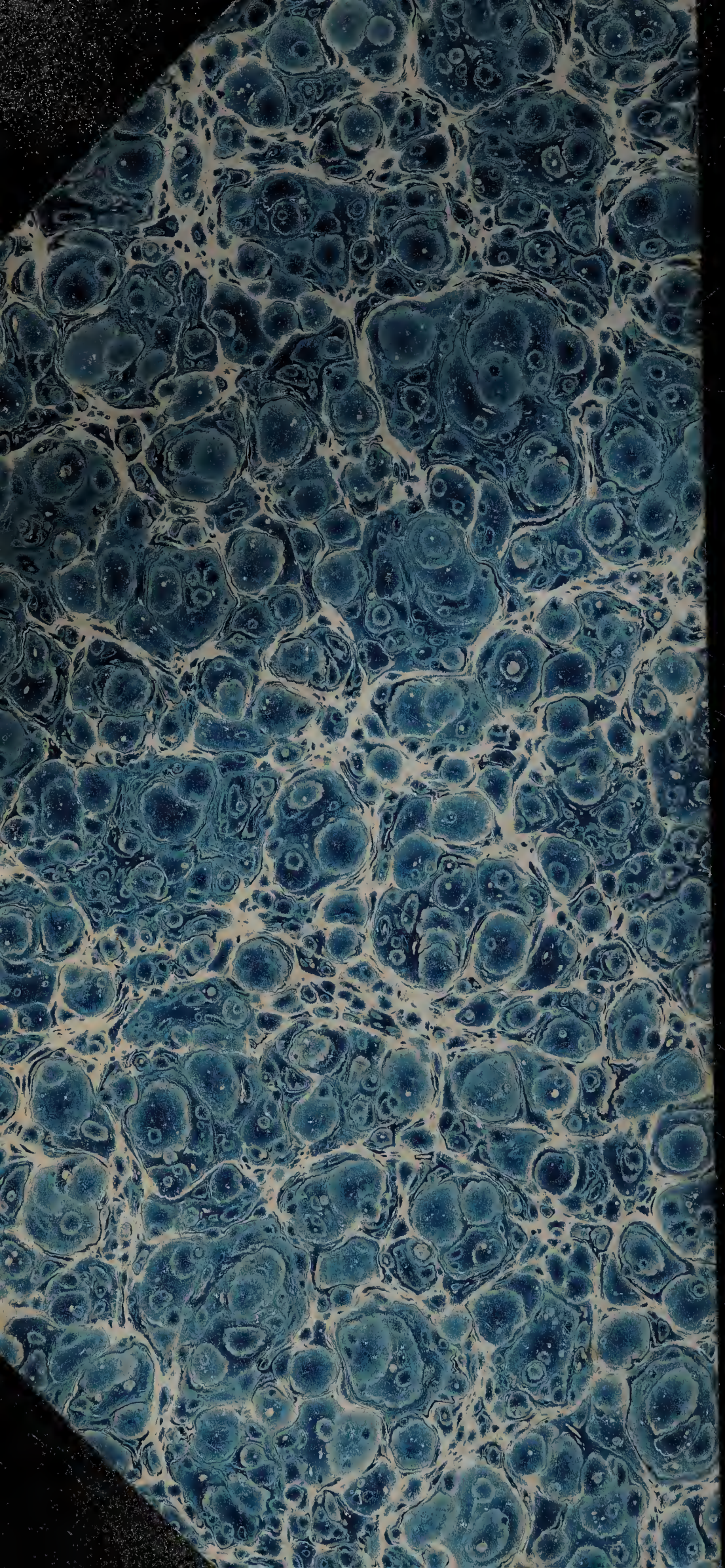
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